JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE

State of Pennsylvania,

DEVOTED TO THE

MECHANIC ARTS, MANUFACTURES, GENERAL SCIENCE,

AND THE RECORDING OF

AMERICAN AND OTHER PATENTED INVENTIONS.

DECEMBER, 1831.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Description of an apparatus, called the Rotascope, for exhibiting several phenomena, and illustrating certain laws of rotary motion. By WALTER R. JOHNSON.—(*With a Plate.*)

OUR common books of mechanics generally contain concise accounts of the doctrines of rotary motion, limited for the most part, however, to the consideration of central forces, the centre of percussion and of gyration, and the centre of spontaneous rotation, to which may be added, the laws of oscillatory motion.

The forces tending to change the position of the axis of rotation are generally either wholly omitted, or if concisely stated in an abstract form, are apparently regarded as incapable of experimental illustrations. The whirling table of Mr. Ferguson is an ingenious apparatus for exhibiting the amount and direction of the several forces exerted by a body in its own fixed plane of revolution. But that instrument makes no provision for the phenomena above referred to.

When we consider that the extensive diffusion of a branch of knowledge, often depends on the facility with which its elements can be made apparent to the understanding, we are at no loss in estimating the practical value of philosophical instruments, whether intended for demonstration or for research. Of this truth the machine of Attwood may be taken as an illustration. This machine gives a most elegant and satisfactory exhibition of the principles of uniform, accelerated, and retarded motions, as dependant on the force of gravity.

All the motions in the machine may be so slow as to reduce the resistance of the air to an unimportant element, and the friction and inertia of the parts being separately determined and allowed for, the theoretical laws of motion are seen to be perfectly confirmed by the experiments.

As to the manner in which the principles of rotation have generally been explained, it may be briefly stated on the plan of what are called rectangular co-ordinates. As, by referring the effect of any force applied opposite to the centre of gravity of a body at rest, to three lines mutually crossing each other at right angles, the resulting direction which the centre of gravity of that body will take in free space, is inferred; so, by a consideration of three perpendicular axes of revolution within the body itself, we may determine the effect of any number of forces tending to produce rotation. Combining these two together we have the resultant motions both of rotation and translation. One of the most important propositions pertaining to the physical character of the subject, is that discovered and demonstrated by Frisi; that "when a body revolves on an axis, and a force is impressed, tending to make it revolve on another, it will revolve on neither, but on a line in the same plane with them, dividing the angle which they contain so that the sines of the parts are in the inverse ratio of the angular velocities with which the body would have revolved about the said axes separately."

The following are among the elementary experiments and observations which led to the construction of this instrument.

1. When we take up by its brazen meridian a common artificial globe, and, having given it a rapid motion about its axis, attempt to move the poles from their position, we shall find our efforts resisted, or the globe impelled in various directions, in a manner which will generally surprise those to whom the experiment is new. If the globe be held by the meridian, at points over the equatorial circle of the sphere, and the axis placed nearly vertical, and in this state of things the revolving globe be carried alternately from the right to left, and the reverse, by the extended arms as in a small orbit, or portion of an orbit, the tendency of the sphere to change the position of its axis will be felt in one or both directions of the movement.

2. There is in use a small apparatus for striking fire, composed of a semicylindrical box of tinned iron, a few inches in length, at one end of which is a small cavity for receiving the tinder, and above it is mounted, on an axis, a disk of steel to strike, when in rapid motion, upon a flint, held just above the tinder. The steel disk is put in motion by the friction of a string drawn briskly over a pulley on the axis. If, when the wheel in this apparatus, (which is about two inches in diameter,) is revolving vertically, we hold the whole loosely in the hand extended, and carry the latter alternately right and left before the body, so as to cause the wheel to describe a horizontal curve, to which the direction of its axis at the commencement of the motion is a tangent, we shall perceive a strong tendency in the wheel to leave the vertical and assume the horizontal position.*

3. A small apparatus, said to have been devised by the celebrated Laplace to illustrate the precession of the equinoxes, has been made in France, and imitated by an ingenious mechanic of Philadelphia.

* This neat little experiment had been made and was first communicated to me by Mr. William Mason, who, to render it the more striking, had mounted the whole box on a pointed axis passing longitudinally through the centre of gravity.

It is formed of two concentric rings revolving on axes at right angles to each other. Within the inner ring is a small spheroid, loaded at one of its poles in such a manner as to produce a rotation in the axis of the inner ring when the spheroid is caused to revolve with rapidity. The chief parts of the rotascope had been devised and constructed before I had an opportunity of seeing the above described apparatus.

4. A number of ingenious experiments were some time ago contrived and executed by Mr. Rufus Tyler, a skilful mechanician of Philadelphia, with an apparatus resembling, in some respects, the common *top;* included in a ring, and placed on a whirling table.

In that arrangement his experiments coincided to a certain extent with some of those which are presented with the rotascope on the orbit-rod.

There was wanting, however, the means of developing and exhibiting the causes which produce the changes which are actually seen to take place. This end is most important in whatever concerns the principles of mechanics. It is what constitutes the great beauty of Attwood's experiments, that the action of gravity is made to coincide, *in principle*, with its actual operation when unrestrained; while, at the same time, the bodies submitted to its action, move with velocities which can be readily followed by the eye.

The following description refers to the accompanying plate:—

A, is a fly wheel, about eight inches in diameter, formed in such a manner as to receive but slight resistance from the air. It is supported on the centre of a perfectly cylindrical axis about $\frac{3}{8}$ of an inch in diameter, terminated by cones to serve as pivot points, on which the wheel runs. The wheel is of brass, the axis of steel, one part, from the wheel towards the pivot, being polished, the other bronzed, for more readily distinguishing the changes of position. The wheel and its axis weigh about 2 lbs. 11 oz.

B, is the base or tripod of mahogany, which sustains the instrument.

F, is a wooden frame containing the principal moving parts of the apparatus.

1, 2, 3 are concentric metallic rings, each about $\frac{3}{4}$ of an inch in breadth, and about $\frac{2}{10}$ of an inch in thickness. The exterior one, (3,) being about fifteen inches exterior diameter, is sustained in its place by the screws *s*, *s*, which have their ends conically excavated to receive the pivot. The axis of the next ring, (2,) is at right angles to that of 3, and again the axis of 1, is at right angles to that of 2, and the axis of the wheel A, to that of the ring 1.

The centre of gravity of the wheel is likewise that of the whole system, and the axis of motion of each ring passes through that centre.

e, is a pivot to the vertical shaft *e*, *f*, upon which the frame F is supported, and upon which it may revolve. The axis of this shaft likewise passes through the centre of the wheel A.

f, is a socket and cone furnished with a tightening screw.

t, is a thumb-screw to fasten and hold the axis *e*, *f*, whenever it becomes necessary to prevent the horizontal motion of the frame F.

p, *p*, are two pulleys attached to the two upright pieces of the frame by metallic bands, and held fast, at any convenient height, on these supports by a screw on the back side; by taking out the screws *s*, *s*, the pulleys may be carried below the axis of the outer ring.

u, *u*, are nuts to keep in place the upper piece of the frame, and having a hole drilled through their heads to receive cords by which the whole frame may be suspended from the ceiling; and *h*, *h*, are two hooks for a similar purpose, and likewise for suspending other weights when not in use.

M, M, are weights acting as moving forces, to set the wheel A in motion. For these weights, the hand of the experimenter may, in many cases, be conveniently substituted, especially where it is not important to know the precise velocity attained. The cord is attached to the axis by means of the small projecting conical knob *k*, to which the centre of the cord is connected by doubling it, applying the pin to the double part, and then setting the wheel in motion to wind up the cord, so that one end will be drawn off from above, the other from below, and both tend to turn the wheel in the same direction.

c, *c*, are two cords connected with the axis of the ring 2, and passing over the pulleys *p*, *p*.

W, W, are weights attached to these cords.

z, indicates the direction in which those weights tend to turn the ring 2 about its axis. This direction is reversed by winding up the cord in the opposite direction.

w, *w*, are weights applied to pulleys firmly connected with the axis of circle 1, and tending to produce a rotation in the direction opposite to that indicated by *v*.

m, is a weight suspended or otherwise attached to the same circle, and tending to produce rotation in the direction of *v*.

x, denotes the direction in which the wheel will move when actuated by the forces M, M, as here represented; but by turning the wheel in the opposite direction, when the cord is applied over the knobs, it will be put in motion in the opposite direction, and the ring 2, will also move opposite to *z*, by the force of *m*.

O, is a bar of mahogany, called the orbit-rod, six feet in length, with a socket, by means of which it may, when the frame F is removed, be placed on the pivot *e*, and made to revolve. In this case the frame containing the wheel is to be set, or suspended, at one end, while at the other is suspended the weight C, which exactly counterpoises the frame and its appurtenances. This weight is placed below the bar in order to bring the centre of gravity as low as practicable, and produce a more stable equilibrium.

The following directions and cautions in using the rotascope will be found useful to those who may not be familiar with its action.

In winding up the *moving cord* around the axis of the wheel, it is necessary to keep the two ends as near to each other as practicable without having one *overlay*, or actually rub against, the other, and to have them wound from beginning to end of the spiral, parallel to each other, without crossings, as the latter will materially obstruct the un-

coiling when the force is applied, and endanger the breaking of the cord.

Care should be taken that the uncoiling be made in such a position of the rings that the moving cord will free itself immediately from all contact with the wheel, at the instant it leaves the shaft.

The cords applied to the several pulleys on the first and second rings, should be kept closely wound round their respective pulleys when not wanted for immediate use, as they may otherwise become entangled in the wheel and obstruct its motion, or essentially endanger the accuracy and safety of the whole instrument.

In using the orbit-rod, the weights should be attached first, then the frame, F, put in its place, and finally, set upon the pivot *e*, when the base, B, will sustain the whole. The revolution should begin with a slow motion, and increase in velocity—all shocks and sudden changes of motion should be avoided.

When it becomes necessary to add any weights to the rings or other parts of the apparatus while on the orbit-rod, an equal weight should be added to the *counterpoise* to avoid lateral pressure on the pivot *e*.

When the *elementary particles* are placed on the axis, the changes of position of the rings should be made gradually, to avoid violent blows of the particles upon the ring 1; otherwise, they may bruise its edge, and be thrown off with violence.

When the wheel is to be stopped, it is most convenient and safe to do it by applying a moderate friction with the fingers to the axis.

To set the wheel in motion, apply the cord round the axis of the wheel, doubling it for that purpose, and putting the fold at the centre over the small pin near the end of the axis. Having wound up the cord, take one end in each hand, and draw the two ends apart with suitable force in directions at right angles to the axis, and as nearly as may be in the plane of the first circle, as well as parallel to the wheel itself.

The following are among the experiments which may be performed by the aid of the rotascope.

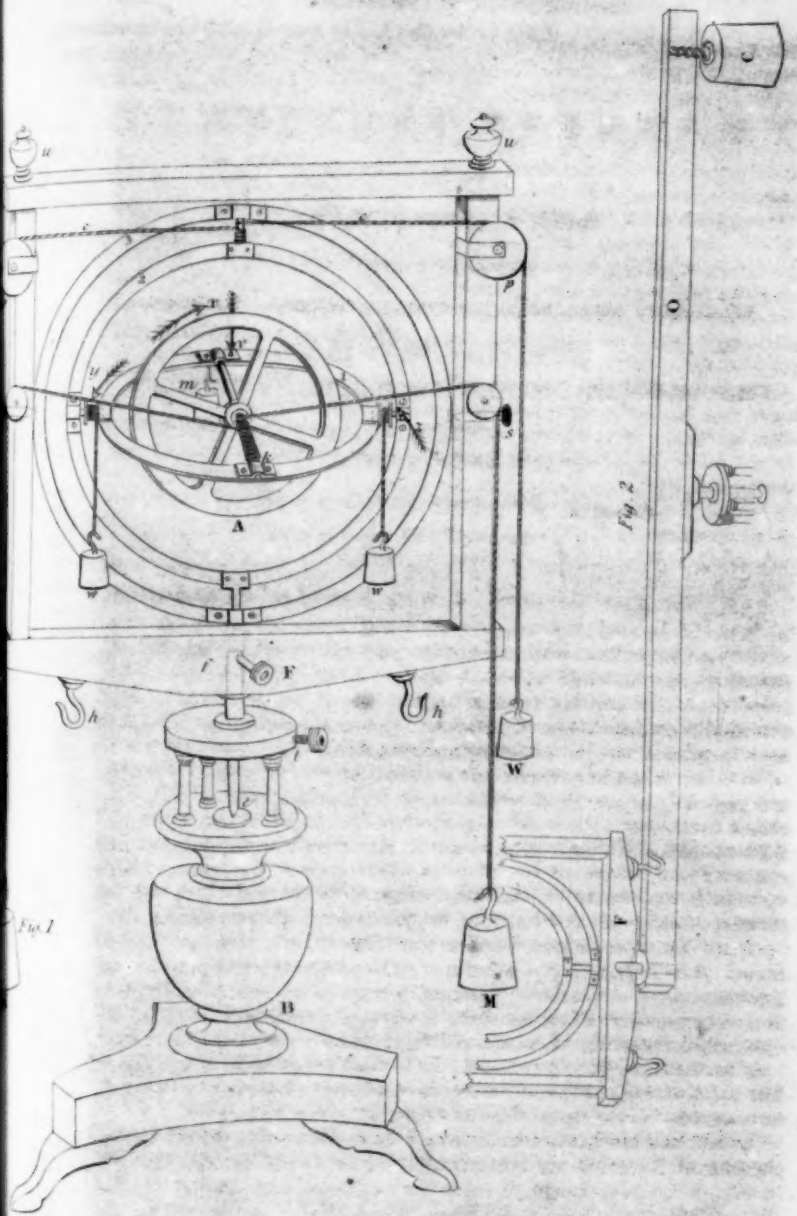
1. Take the wheel and its supporting circle from the frame. Connect with the ring, at a point opposite to the axis of the wheel, a wooden rod of sufficient strength to bear the weight when held horizontally, and from nine to twelve inches in length. Attach the end of this rod, remote from the wheel, to a cord suspended from the ceiling. Set the wheel in rapid motion, and then bring it up so that the rod shall be horizontal. Then suddenly abandoning it with the hand, the cord will sustain it, but instead of hanging vertically down, the axis of the wheel and rod, which may be regarded as its prolongation, will be kept for some time horizontal; but though thus suspended, as if by some mysterious agency, they constantly perform a circuit which has an *imaginary vertical* drawn from the point of suspension, for its axis. If the velocity of the horizontal revolution be diminished, the sustaining rod will incline downwards more rapidly than when left to itself, until at length it reaches the position of rest. But if the velocity of revolution be augmented by any external force, the wheel and ring will rise in opposition to gravity until the rim of the

wheel strikes the suspending cord. The wooden rod will then have come to a position nearly vertical, sustaining the wheel and ring at its upper end, but still continuing the horizontal motion. This paradoxical appearance, would continue the longer by having a delicate metallic swivel link in some part of the cord, which would prevent the twist that otherwise soon opposes the horizontal motion to such an extent as to depress the rod in the course of a few minutes. It will be seen that the revolution in a horizontal direction, being the resultant of gravity, combined with the rotary motion of the wheel, must become more rapid in proportion as the velocity of the latter on its axis is diminished: because the force of gravity is then a greater component in the combined forces which act upon the system.

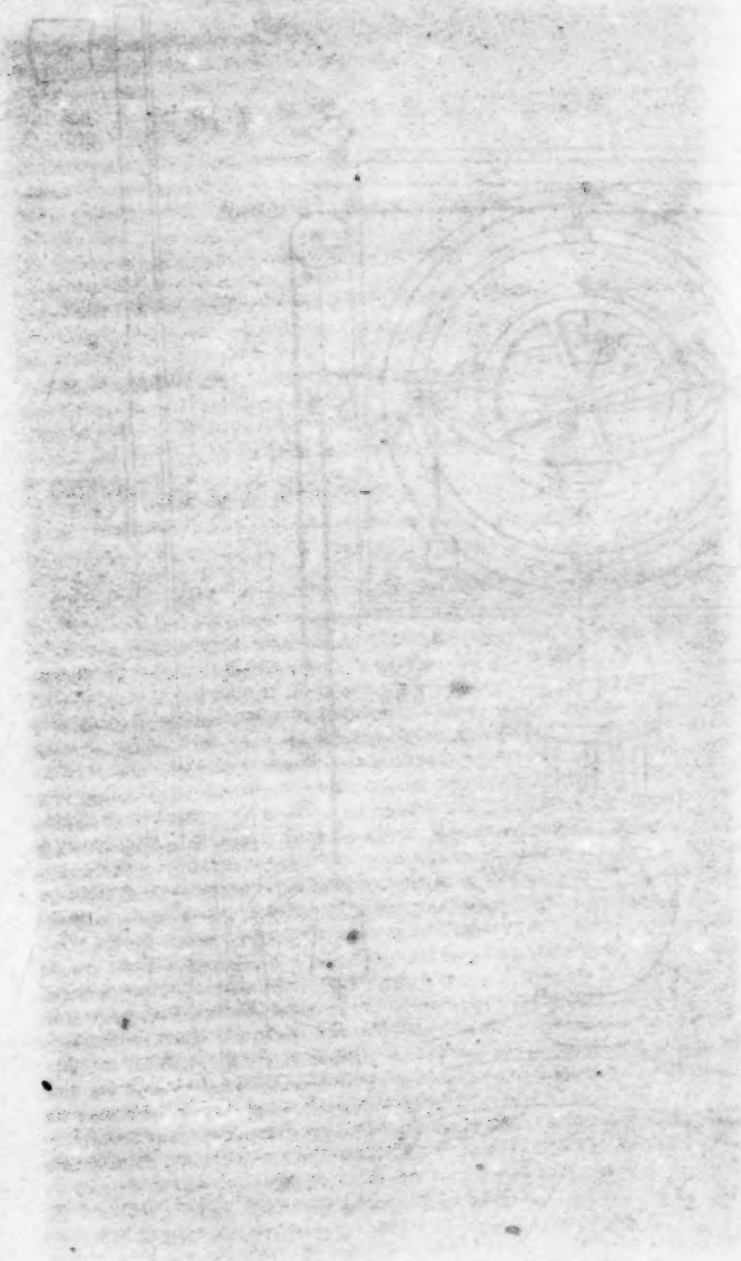
2. Having replaced the wheel and ring, in their connexion with the frame, set the latter on its pivot upon the base, B. Make the circle, or ring, 3, fast in a vertical position, apply cords to the pulley on the axis of ring 2, and bringing the pulleys *p*, *p*, to a proper elevation; make them fast, and pass those cords over them to sustain weights. Having given the wheel a rapid motion, take hold of one of the urns *u*, *u*, and cause the whole frame to revolve horizontally on its pivot. As the persistency of the wheel in the plane of its motion prevents the ring 2 from revolving, the motion of the frame will gradually wind up the cords about the pulley. At the same time, however, the ring 1 will gradually change its plane, and bring the wheel to a position to obey the action of the weights. The portion of cord which had been previously wound about the pulley will then be uncoiled, and a considerable momentum communicated to the system composed of the rings 1 and 2, which will, if the tightening screw, *t*, be made fast, again wind up the cord in the opposite direction about the pulley. As soon as the said rings, however, are again deprived of their momentum by the action of the weights, the latter will again tend to produce a rotation in the ring 2, which will be opposed by the persistency of the wheel. If, at this moment, the pivot be released from the screw, the whole system, composed of the two weights, the frame, and ring 3, will be made to revolve by the gravity of the weights, while the ring 2 remains pertinaciously fixed in its position, until the wheel has had time again to invert its axis. This time will be greater or less according to the greater or less velocity of rotation in the wheel, compared with the size of the weights hanging over the pulleys.

3. Set, or suspend, the frame on the orbit rod. Give the wheel a moderate velocity of rotation, and set the whole in motion upon the pivot *e*, all the circles being free to move on their respective axes. In whatever direction the wheel revolves, with respect to the plane of the orbit, at the commencement of the *orbicular* revolution, it will soon be observed to conform in direction to the latter. If *this* be reversed, *that* will soon be reversed also.

4. Repeat the third experiment with only the addition of a weight of eight ounces attached to the second circle, opposite to the axis of the first. The effort of the wheel to take and maintain in its rotation the direction of this orbicular motion, will be sufficient to keep the weight elevated nearly to a level with the centre of the wheel.



ROTASCOPE BY W. R. JOHNSON.



Continuation of the Report of the Committee of the Franklin Institute of Pennsylvania, appointed May, 1829, to ascertain, by experiment, the value of Water as a Moving Power.

(Continued from p. 305.)

TABLE L.—PART I.

CHUTE No. 5.—*Elbow buckets. Close breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Experi.	Head of water above.			Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	Worthless expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Bim. of gate.	Top of bkt.	Bim. of bkt.															
	Feet.	Feet.	Feet.	In.	Pds.	Pounds.	Pounds.	Feet.	Scds.	Feet.	Pds.	Feet.						
1	7.75	9.33	10.41	1.50	566	48.95	614.95	41.5	33	11.84	5425	14.75	800187	255204	.319			
2					669	50.58	719.58		36	10.86	6075		896062	298624	.333	.333	10.86	
3					772	52.21	824.21		42	9.31	7025		1036187	342046	.330			
4					875	53.84	928.84		49	7.98	8175		1206812	385468	.319			
5	7.00	8.58	9.66	1.50	566	48.95	614.95	51.5	34	11.50	5505	14.00	770700	255204	.330			
6					669	50.58	719.58		38	10.28	6230		872200	298624	.342	.342	10.38	
7					772	52.21	824.21		44	8.88	7225		1011500	342046	.338			
8	7.00	8.58	9.66	1.75	566	48.95	614.95	41.5	32	12.20	5350	14.00	749000	255204	.340			
9					669	50.58	719.58		34	11.50	5925		829500	298624	.359			
10					772	52.21	824.21		39	10.02	6735		942900	342046	.362	.362	10.02	
11					875	53.84	928.84		45	8.71	7880		1103200	385468	.349			
12					978	55.47	1033.47		51	7.66	8880		1243200	428889	.345			
13	6.00	7.58	8.66	1.50	566	48.95	614.95	41.5	40	9.77	5700	13.00	741000	255204	.344			
14					669	50.58	719.58		41	9.54	6300		819000	298624	.364	.364	9.54	
15					772	52.21	824.21		49	7.98	7525		978250	342046	.349			
16	6.00	7.58	8.66	1.75	669	50.58	719.58	41.5	37	10.57	6075	13.00	789750	298624	.378	.378	10.57	
17					772	52.21	824.21		44	8.88	7050		916500	342046	.373			
18					875	53.84	928.84		50	7.82	8230		1069900	385468	.360			
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18

Table L.—Part II.

Chute No. 4.—*Elbow buckets. Close breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Exper't.	Head of water above.			Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	W't of water expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Bttm. of gate.	Top of bkt.	Bttm. of bkt.															
	Feet.	Feet.	Feet.	In.	Pds.	Pounds.	Pounds.	Feet.	Secs.	Feet.	Pds.	Feet.						
19	6.00	7.58	8.66	2.00	566	48.95	614.95	41.5	31	12.60	5400	13.00	702000	255204	.363			
20					669	50.58	719.58		35	11.16	5950		773500	298624	.386	.386	11.16	
21					772	52.21	824.21		40	9.77	6950		903500	342046	.378			
22	5.00	6.58	7.66	1.25	566	48.95	614.95	41.5	46	8.50	5850	12.00	702000	255204	.363	.363	8.50	
23					669	50.58	719.58		56	6.98	7165		859800	298624	.347			
24	5.00	6.58	7.66	1.50	669	50.58	719.58	41.5	46	8.50	6490	12.0	778800	298624	.383	.383	8.50	
25					772	52.21	824.21		55	7.10	7950		954000	342046	.359			
26	5.00	6.58	7.66	1.75	772	52.21	824.21	41.5	49	7.98	7510	12.00	901200	342046	.379	.379	7.98	
27					875	53.84	928.84		58	6.74	8580		1029600	385468	.374			
28	5.00	6.58	7.66	2.00	669	50.58	719.58	41.5	39	10.02	6135	12.00	736200	298426	.405	.405	10.02	
29					875	53.84	928.84		54	7.24	8425		1011000	385468	.381			
30	4.00	5.58	6.66	1.00	360	46.61	406.61	41.5	46	8.50	4525	11.00	497750	168743	.339			
31					463	48.48	511.48		54	7.24	5350		588500	212264	.360	.360	7.24	
32					566	50.35	616.35		68	5.75	6500		715000	255785	.357			
33	4.00	5.58	6.66	1.50	463	48.48	511.48	41.5	38	10.28	5050	11.00	555500	212264	.382			
34					566	50.35	616.35		43	9.09	5675		624250	255785	.409	.409	9.09	
35					669	52.22	721.22		52	7.52	6800		748000	299306	.400			
36					772	50.09	826.09		64	6.10	8350		918500	342827	.373			
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	

Table L.—Part III.

Chute No. 5.—*Elbow buckets. Close breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Exper't.	Head of water above. Bttm. of gate.	Head of water above. Top of bkt.	Head of water above. Bttm. of bkt.	Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	Wt of water expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Feet.	Feet.	Feet.	In.	Pds.	Pounds	Pounds.	Feet.	8cds.	Feet.	Pds.	Feet.						
37	3.00	4.58	5.66	1.50	463	48.48	511.48	41.5	41	9.54	5000	10.00	500000	212264	.425			
38					566	50.35	616.35		49	7.98	5800		580000	255785	.441	.441	7.98	
39					669	52.22	721.22		57	6.86	6900		690000	299306	.433			
40	3.00	4.58	5.66	1.75	566	50.35	616.35	41.5	44	8.88	5575	10. 0	557500	255785	.459	.459	8.88	
41					669	52.22	721.22		52	7.52	6650		665000	299306	.450			
42					772	54.09	826.09		64	6.10	8100		810000	342827	.423			
43	3.00	4.58	5.66	2.00	566	50.35	616.35	41.5	42	9.31	5500	10.00	550000	255785	.465	.465	9.31	
44					669	52.22	721.22		51	7.66	6700		670000	299306	.447			
45	2.00	3.58	4.66	1.00	257	44.74	301.74	41.5	51	7.66	3125	9.00	281250	125222	.445			
46					360	46.61	406.61		66	5.92	4025		362250	168743	.466	.466	5.92	
47					463	48.48	511.48		85	4.60	5225		470250	212264	.451			
48	2.00	3.58	4.66	1.25	257	44.74	301.74	41.5	40	9.77	3800	9.00	342000	125222	.366			
49					463	48.48	511.48		60	6.50	5175		465750	212264	.455	.455	6.50	
50					566	50.35	616.35		67	5.84	6450		580500	255785	.440			
51					669	52.22	721.22		81	4.83	7675		690750	299306	.433			
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	

Table L.—Part IV.

Chute No. 5.—*Elbow buckets. Close breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Exper't.	Head of Water above.			Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	Wt of water expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Btm. of gate.	Top of bkt.	Bum. of bkt.															
	Feet.	Feet.	Feet.	In.	Pds.	Pounds	Pounds.	Feet.	Scds.	Feet.	Pds.	Feet.						
52	2.00	3.58	4.66	1.50	360	46.61	406.61	41.5	42	9.31	4350	9.00	391500	168743	.431			
53					463	48.48	511.48		50	7.82	5150		463500	212264	.458			
54					566	56.35	616.35		59	6.62	6150		553500	255785	.462	.462	6.62	
55					669	52.22	721.22		72	5.43	7425		668250	299306	.448			
56	2.00	3.58	4.66	1.75	463	48.48	511.48	41.5	45	8.71	5125	9.00	461250	212264	.460			
57					566	50.35	616.35		55	7.10	6025		542250	255785	.471	.471	7.10	
58					669	52.22	721.22		66	5.92	7250		652500	299306	.459			
59					772	54.09	826.09		78	5.01	8600		774000	342827	.443			
60	2.00	3.58	4.66	2.00	463	48.48	511.48	41.5	45	8.71	5075	9.00	456750	212264	.464			
61					566	50.35	616.35		54	7.24	6000		540000	255785	.473	.473	7.24	
62					669	52.22	721.22		64	6.10	7150		643500	299306	.465			
63	1.00	2.58	3.66	2.00	360	46.61	406.61	41.5	53	7.38	4000	8.00	320000	168743	.527			
64					463	48.48	511.48		65	6.00	4930		394400	212264	.539	.539	6.00	
65					566	50.35	616.35		82	4.77	6025		482000	255785	.530			
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	

TABLE M.—PART I.

CHUTE No. 5.—*Centre buckets. Close breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Exper't.	Head of water above.			Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	Wt of water expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Btm. of gate.	Top of bkt.	Btm. of bkt.															
	Feet.	Feet.	Feet.	In.	Pds.	Pounds	Pounds.	Feet.	Scds.	Feet.	Pds.	Feet.						
1	7.75	9.33	9.33	1.00	257	44.06	301.06	41.5	30	13.00	3675	14.75	542062	124950	.230			
2					360	45.69	405.69		33	11.84	4125		608437	168361	.276			
3					463	47.32	510.32		38	10.28	4625		682187	211782	.310			
4					669	50.58	719.58		48	8.14	5050		862875	298624	.345			
5					772	52.21	824.21		53	7.38	6500		958750	342046	.356	3.56	7.38	
6					875	53.84	928.84		61	6.40	7425		1095187	385468	.351			
7					978	55.47	1033.47		75	5.21	8775		1294312	428889	.331			
8	7.75	9.33	9.33	1.25	669	50.58	719.58	41.5	37	10.56	5475	14.75	807562	298624	.368			
9					772	52.21	824.21		40	9.77	6050		892375	342046	.383			
10					875	53.84	928.84		44	8.88	6685		986037	385468	.390	.390	8.88	
11					978	55.47	1033.47		50	7.82	7525		1109937	428889	.386			
12					1081	57.10	1138.10		56	6.98	8450		1246375	472310	.378			
13	7.75	9.33	9.33	1.50	772	52.21	824.21	41.5	35	11.16	5820	14.75	858450	342046	.398			
14					875	53.84	928.84		38	10.28	6375		940312	385468	.410			
15					978	55.47	1033.47		43	9.09	7050		1039875	428889	.412	.412	9.09	
16					1081	57.10	1138.10		47	8.32	7875		1161562	472310	.407			
17	7.75	9.33	9.33	1.75	875	53.84	928.84	41.5	37	10.56	6250	14.75	921875	385468	.418			
18					978	55.47	1033.47		38	10.28	6725		991937	428889	.432	.432	10.28	
19					1081	57.10	1138.10		42	9.31	7500		1106250	472310	.427			
20					1184	58.73	1242.73		46	8.50	8325		1227937	515733	.420			
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	

Table M.—Part II.

Chute No. 5.—*Centre buckets. Close Breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Exper't.	Head of water above.			Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	Wt of water expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Btm. of gate.	Top of bkt.	Btm. of bkt.															
	Feet.	Feet.	Feet.	In.	Pds.	Pounds	Pounds.	Feet.	Scds.	Feet.	Pds.	Feet.						
21	5.00	6.58	6.58	0.75	463	47.32	510.32	41.5	56	6.98	4830	12.00	579600	211782	.365			
22					566	48.95	614.95		67	5.84	5700		684000	255204	.373			
23					669	50.58	719.58		86	4.54	6600		792000	298624	.377	.377	4.54	
24	5.00	6.58	6.58	1.00	566	48.95	614.95	41.5	48	8.14	5375	12.00	645000	255204	.395			
25					669	50.58	719.58		55	7.10	6050		726000	298624	.411			
26					772	52.21	824.21		63	6.20	6885		826200	342046	.414	.414	6.20	
27					875	53.84	928.84		72	5.43	7950		954000	385468	.404			
28					978	55.47	1033.47		84	4.65	9215		1105800	428889	.387			
29	5.00	6.58	6.58	1.25	669	50.58	719.58	41.5	44	8.88	5775	12.00	693000	298624	.430			
30					772	52.21	824.21		48	8.14	6460		775200	342046	.441			
31					875	53.84	928.84		55	7.10	7265		871800	385486	.442	.442	7.10	
32					978	55.47	1033.47		62	6.30	8250		990000	428889	.433			
33	5.00	6.58	6.58	1.50	772	52.21	824.21	41.5	43	9.09	6250	12.00	750000	342046	.456			
34					875	53.84	928.84		49	7.98	7000		840000	385468	.458	.458	7.98	
35					978	55.47	1033.47		55	7.10	7875		945000	428889	.453			
36	5.00	6.58	6.58	1.75	772	52.21	824.21	41.5	40	9.77	6200	12.00	744000	342046	.459			
37					875	53.84	928.84		45	8.70	6800		816000	385468	.472	.472	8.70	
38					978	55.47	1033.47		50	7.82	7660		919200	428889	.461			
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	

TABLE M.—PART III.

CHUTE No. 5.—*Centre buckets. Close breast. Bottom of gate 7 feet above bottom of wheel.*

No. of Exper't.	Head of water above. Btm. of gate	Head of water above. Top of bkt.	Head of water above. Btm. of bkt.	Width of Aperture.	Weight raised.	Friction.	Sum of friction and weight raised.	Height raised.	Time.	Velocity per second.	Wt of water expended.	Head and fall.	Power.	Effect.	Ratio, power being 1.	Maximum effect.	Velocity at maximum.	Observations.
	Feet.	Feet.	Feet.	In.	Pds.	Pounds	Pounds.	Feet.	Scds.	Feet.	Pds.	Feet.						
39	2.00	3.58	3.58	0.75	257	44.74	301.74	41.5	62	6.30	3025	9.00	272250	125222	.460			
40					360	46.61	406.61		78	5.01	3955		355950	168743	.474	.474	5.01	
41					463	48.48	511.48		105	3.72	5200		468000	212264	.453			
42	2.00	3.58	3.58	1.00	360	46.61	406.61	41.5	51	7.66	4200	9.00	378000	168743	.446			
43					463	48.48	511.48		62	6.30	5000		450000	212264	.471			
44					566	50.35	616.35		71	5.50	5840		525600	255785	.486	.486	5.50	
45					669	52.22	721.22		83	4.71	6850		616500	299306	.485			
46					772	54.09	826.09		102	3.83	7900		711000	342827	.482			
47	2.00	3.58	3.58	1.25	463	48.48	511.48	41.5	51	7.66	4850	9.00	436500	212264	.486			
48					566	50.35	616.35		58	6.74	5555		499950	255785	.511			
49					669	52.22	721.22		68	5.75	6450		580500	299306	.516	.516	5.75	
50					772	54.09	826.09		77	5.07	7450		670500	342827	.511			
51					875	55.96	930.96		88	4.44	8640		777600	386349	.496			
52	2.00	3.58	3.58	1.50	566	50.35	616.35	41.5	53	7.38	5450	9.00	490500	257785	.525			
53					669	52.22	721.22		62	6.30	6260		563400	299306	.531	.531	6.30	
54					772	54.09	826.09		71	5.50	7225		650250	342827	.525			
55					875	55.96	930.96		82	4.77	8340		750600	386349	.515			
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	

[TO BE CONTINUED.]

FRANKLIN INSTITUTE.

Explosions of Steam Boilers.

Continued from p. 315.

(No. XI.)

Letter from L. Hebert, Esq., of London, in reply to the Circular of the Committee of the Franklin Institute.

20 *Paternoster Row, London*, 12 *October*, 1830.

ALTHOUGH I cannot have the satisfaction of furnishing you with any very important evidence on the subject, which the committee of your valuable institution have so laudably undertaken to investigate; yet, with the hope that the addition of my "mite," may tend in some degree to promote the object you have in view, I venture to give you a brief detail of my limited experience and observation; to which I shall take leave to add a few suggestions for improving the apparatus connected with the generation of steam.

It has never fallen to my lot to be present during the explosion of an *ordinarily constructed* steam-engine boiler, but I have seen the condition of several immediately, or soon after, that occurrence, and have obtained information upon which I can rely, respecting many of the circumstances attending them. I have, however, "stood fire" several times in the experimental disruption of tubular boilers, from a conviction of their innocuous effects, which I shall more particularly notice in the course of this letter.

With respect to the common capacious boilers, the first instance I shall mention was that of a long cylindrical boiler, distinguished in England by the name of Trevithick, as the presumed inventor,* in which three separate burstings took place in precisely the same part of the boiler;—in that part immediately above the strongly ignited fuel of the furnace, and extinguishing the fire each time, but without causing any further material damage: the pressure in each case being upwards of fifty pounds upon the inch. The boiler was made of the best malleable iron. Previous to the disruption there was observed a bulging, or swelling out, of the metal, which gradually increased till it became nearly of a hemispherical figure, when it burst open and let the water out of the boiler into the fire. The boiler was repaired by putting a thick patch of malleable plate iron over the hole, when, after about six weeks wear and exposure to the fire, this metal bulged out again, and burst asunder; a third patch was substituted, and in about a similar period of time, was destroyed in the like manner. As the third disaster might naturally be expected from the results experienced in the first and second, it is proper to notice that the urgent wants of the manufactory were such as to render it expedient to get the engine into working order, notwithstanding that circumstance. The cause of these ruptures appeared upon investigation to be owing

* I am inclined to think that your countryman, Oliver Evans, was the earliest inventor of this long boiler; if so, the merit of our justly celebrated Trevithick, will, in *this* instance, be merely that of having spoiled "a good original," by his having increased the diametrical proportions of the cylinder.

to a partial and very intense heat impinging against that particular spot where they took place. A positive proof of this might very likely have been afforded by an alteration in the flue, but the working of the engine was, after the last accident, discontinued. This boiler was not set with the furnace in its usual situation, but underneath it, at one end; the heated air and flame, consequently, after impinging against the bottom, suddenly turned off at right angles to the remotest end of the boiler where it first entered the internal flue, and the air for combustion was supplied by means of what the masons here call an *air-drain;* consisting of a narrow subterraneous channel leading from the external atmosphere, (that is, outside of the building,) into the ash pit, where it terminated in a small opening near to, and directly under, the furnace bars; consequently, producing the effect of a continuous blast upon that part of the boiler where the current of air first impinged.

I am strengthened in this opinion by the information derived from a friend, that Mr. John Martineau, (a respectable engineer of London,) had a boiler which was twice destroyed in the same spot; upon endeavouring to ascertain the cause, Mr. Martineau discovered a fissure in the brick-work exactly opposite that part of the boiler where the ruptures took place; through this fissure the air rushed with great impetuosity, producing the effect of a blow-pipe upon the metal.

By a reference to the description of the boiler adopted by Messrs. Braithwaite and Ericsson, (inserted in the "Journal of the Franklin Institute,") in the "Novelty" steam carriage, it will be perceived that that part of their boiler where the flue first takes a horizontal course is exceedingly exposed to the destructive influence of the fire, especially when the strong blast they employ is superadded. For having pointed out the imminent risks of explosions incurred by this arrangement of the flue, my motives were impugned and misrepresented; although my intention was chiefly to show the cause of an acknowledged effect, that might have been attended with very serious disaster. My reason for mentioning this circumstance here, is, that as Messrs. Braithwaite and Ericsson's boiler has been trumpeted forth by the British press, generally, as a perfect model for imitation, it may come under the notice of your committee, who will probably decide, whether the arrangement of the parts, (however excellent in some respects,) is not calculated to cause an early disruption of that part of the boiler, where the flue first takes a horizontal course? I beg leave also to submit an opinion to your committee, that in forming the flues of steam boilers generally, all sudden bends should be avoided as dangerous, especially such as cause the current of flame and heated gases to strike partially against the boiler; and if a blowing apparatus be used, that the air should be uniformly distributed over the ash-pit, before it reaches the furnace bars;—and in the case of using an exhausting apparatus in the chimney, the same attention should be paid to a uniform distribution of the air, so as to avoid all partial currents.

Although boilers that are constructed with flues running through them, seem to be well contrived for economising heat, (and it is on this account, I conjecture, that they have received so general a pre-

ference,) there results from this arrangement a source of danger, from the deposit of soot becoming ignited. I make no doubt that many boilers have been destroyed owing to this circumstance, and I can instance one case in which this was *ascertained* to have been the cause: it was that of a high pressure boiler at Adams' Forge, Wednesbury, in Staffordshire, which exploded a few years ago, killing the proprietor, Mr. Adams, and five of his men, besides dreadfully injuring six other persons. The steam in the boiler was at the usual working pressure of sixty pounds, the safety valve was in good order, and there was plenty of water in the boiler. The flue from the furnace, before entering the chimney, passed through the steam chamber in the upper part of the boiler, where a quantity of soot was collected, which having been ignited, caused the surrounding metal to become red hot. In consequence, so sudden and powerful an expansion of the high pressure steam took place, as to render the aperture of the safety valve inadequate to carry off the steam as fast as it was generated, and the catastrophe ensued. The boiler, although formed of the best malleable iron plates, was separated into about fifty parts, which were scattered in all directions and to great distances. This single fact seems to me to place the *danger* of such an arrangement of parts in a pretty clear point of view, and I think we may therefore safely conclude that the plan is at least ineligible.

I shall now, sir, mention a fact that came under my observation, which shows the unfitness of boilers of large capacity or surface being employed for locomotive purposes, (wherein the steam is of no practical use, unless of very great elastic force,) and likewise the positive necessity of having a *principal* safety valve placed out of the control of the attending engineer, who is, naturally, very apt, (as I have had occasion frequently to notice,) to become excited at a moment of difficulty to do very imprudent things.

About three years ago, I was invited to attend an excursion of Messrs. Burstall and Hill's locomotive steam coach, from Lambeth, near London, but I declined being present, expressing my fears of the safety of the boiler. Several of my acquaintances, who did attend, were brought home wounded from the explosion of the boiler, and one of them nearly fatally by a large piece of the boiler striking him, and he was necessarily deposited in the nearest hospital. The wheels of the carriage had got into some loose ground, and the power of the engines appearing inadequate to extricate them, the engineer leaned with his weight upon the safety valve to get an accumulation of power for the purpose. At this time the boiler gave, to the persons surrounding the machine, alarming indications of its weakness, which induced many of them to retire from it; but a friend of mine who was behind it, had the boldness to advance and throw wide open the furnace door; the act was barely completed when the explosion took place, and he was both lacerated and scalded; none were "killed-off," (to use the *humane* expression of one of our now killed-off Ministers of War.) This boiler, like the last mentioned, was made of the best wrought or malleable iron, yet it flew into *pieces*, in spite of those wiseacres who assert that boilers of such iron "only *tear* open."

It was of a circular figure, very shallow, with a dome top, the interior being braced together by iron bars, upon the principle of trussing roofs in architecture.

The most frequent cause of disaster, has been, I believe, a deficiency of water in the boiler, owing to the failure, or imperfect action of the pumps. Several instances of this have come to my knowledge, but I am not in possession, at present, of the particular circumstances attending them, and shall therefore only mention one; which was that of a wrought iron boiler, with a spherical top, of Boulton & Watt's construction, employed at Aston Forge, near Birmingham. In this case it was ascertained that the explosion was caused by the boiler becoming red hot, into which the water was subsequently injected by the force pump, producing so instantaneous and powerful a volume of steam, as to overcome the resistance of the vessel.

Your committee is doubtless fully acquainted with all the facts that have appeared in print, respecting the explosion of steam boilers in this country. I allude, in particular, to the evidence given before committees of the British House of Commons appointed for that purpose.

Although the fates have never permitted me to be present during the explosion of a *common* boiler, (by which I mean all those having vessels of large capacity,) they have allowed me repeatedly to gratify my curiosity by observing the innocuous effects of the bursting of the small distinct chambers of tubular boilers, produced by the forcing of water into them, when the metal was at a bright red, or approaching to a white, heat. The explosion, though alarming to a stranger at first, from the noise and violent ejection of the vapour, is such as to give him confidence to "stand fire" in subsequent experiments. In all those that I witnessed, a *rending* of the metal was uniformly produced, of from one to three or four inches in length, and it usually occurred in that part of a tube where the welding had been more or less imperfectly performed, or where, from other causes, the metal was reduced in thickness. The internal diameter of the tubes in question was barely one inch, the thickness three-sixteenths, generally, but in the weakest places about an eighth of an inch. Although the force of steam to produce such effects might safely be estimated at several hundred pounds upon the inch, as a minimum, the maximum cannot be ascertained without knowing how far the tenacity of the metal was impaired by the great heat to which it was subjected. But such results as attended the bursting of those tubes demonstrated, that under no circumstances of neglect on the part of the engineer, (or even of malicious intent, supposing that to be possible,) or of accidental derangement in the supply pump, or of the safety valve, &c., is any personal danger, whatever, incurred by the use of boilers made with them.

In the case of a boiler of the ordinary proportions and capacity becoming red hot by the temporary suspension of the supply of water, which is afterwards renewed, the destructive effect of an explosion, as compared with that of the tubular boiler before mentioned, would be in the exact proportion of its increased diameter; hence, the conse-

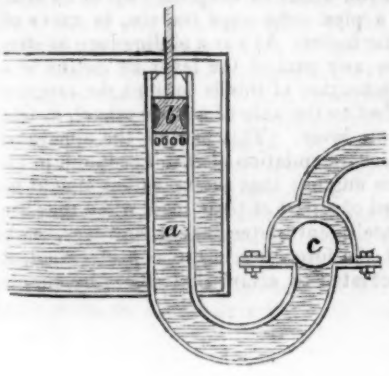
quences, as respects the scalding water, would be as the area of their respective sections, the length being presumed to be the same in both cases. If, therefore, we consider the area of the inch tubes as .785, and estimate the diameter of a common boiler to be 24 inches only, this gives an area of 452.39 inches; consequently, (as will be found upon calculation,) there would be ejected, by an explosion of the latter, 576 times the quantity of water of the tubular boiler, and with a greatly increased momentum. In reply to this, it might be stated that the whole contents of the tubular boiler would be discharged through the fissure opened; but supposing that no provision were made to prevent such a consequence, the jet of water would be nearly like that of a small fountain; whereas, in the large vessel of the common boiler, the accumulated force would produce a sudden and instantaneous irruption of the whole contents, almost in a body, besides projecting great pieces of metal about. It is worthy also of particular notice, that as the comparative safety of boilers is in the inverse ratio of their diameter, the 24 inch boiler must be 24 times as thick as the inch tubular boiler, to sustain the same pressure; that is 4½ inches thick! It is needless to remark, that a boiler of such a thickness would be worse than useless, supposing it practicable to make it.

As the heating of boilers *uniformly* over their surfaces is an object of the utmost importance, your committee will probably think that the plan of heating, patented and adopted by Messrs. Beale and Porter, of London, is worthy of their attentive consideration. It consists in communicating the heat from the furnace through the medium of a surrounding bath, containing a fluid that requires a higher temperature to vapourize it than water; and by varying the composition of the fluid medium, according to the temperature required in the steam, no excess of heat in the engine boiler can take place; nor can any surplus caloric be retained in the bath, as an open pipe proceeds from it to the atmosphere, to carry off whatever vapour may be there produced. The engine boiler is thus protected from any heat that can injure the tenacity of the metal, which need not be of much more substance than is necessary to sustain the working pressure in the boiler. These are the leading advantages set forth by the patentees; but the real merits of the plan, whatever they may be, will, I make no doubt, be duly examined and estimated by your intelligent committee. (An account of Messrs. Beale and Porter's plan for heating high pressure boilers will be found at page 39, vol. V., N. S. of the "Register of Arts and Journal of Patent Inventions.")

One of the fruitful causes of explosions in high pressure steam boilers has been the imperfect action of the force pumps, owing to some derangement of the valves, which they are extremely liable to, on account of the strains to which they are subjected. For this reason the utmost possible simplicity of parts, and the most solid and accurate workmanship, are indispensable. Fine particles of sand insinuating themselves between the surfaces that move in contact very soon render the pumps unserviceable; to prevent which it is highly desirable to use *filtered* water; and the cistern into which the filtered

water is delivered, should, I think, have at least two pipes of communication with the "service pipe," and each of the two pipes should contain a moveable box, in which are properly packed the filtering substances, so that either of them may be renewed at pleasure, without impeding the operation of the other. The cocks and union joints required to render this process convenient, are too obvious to need mentioning. Whilst I am writing, a thought occurs to me, of which possibly something may be made by the ingenious members of your committee; but as I am somewhat dubious of its practicability myself, I put the idea down in the form of a question.—Could not the filtering boxes just mentioned be so arranged as to be rendered *chemically*, as well as mechanically, subservient to the purification of the water,—supposing the filtration to be performed by ascension, and the first, or lowest, stratum of matter, whether solid or fluid, passed through, to be composed of one or more chemical agents, adapted to precipitate the principal matters held in solution by the water? Would not the water thus purified, prevent deposits in the boiler, from whence it is so difficult and troublesome to dislodge it by the ordinary practice? It would surely be much easier to abstract the foreign matter obtained by the first process, in the boxes, than by the last, of evaporation, in the boiler; and I submit that it had better be only partially done, than not at all. The stratum of earthy matter that usually forms and adheres to the bottoms and sides of boilers, not only causes a great loss of heat, or *waste of fuel*, but by interposing a non-conducting substance between the metal and the water, the fire acts very *destructively upon the metal*. Every member of your committee is, I am persuaded, fully aware of these circumstances, and I only introduce a notice of them here, that they may not escape their attention during their important investigation.

With respect to force pumps, so sensible am I of the advantages of simplicity and strength, that I have been led to give a preference to a force pump suggested by Sir James C. Anderson, (in a conversation I had with him on their common defects,) although it works against the pressure of the atmosphere in its up-stroke; of this pump I annex a sketch that I trust will be sufficiently explanatory.

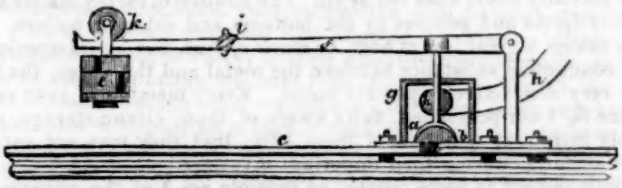


The water enters through the perforations in the tube *a*, when the plunger, *b*, is lifted into the position represented; and when the plunger makes the return stroke, a metallic ball, *c*, is raised, and a quantity of water, equal to that contained in the tube between the perforations and the lowest point of the stroke, is discharged into the boiler. The ball being forced against its seat by the pressure of the steam, prevents

the return of the fluid, and as there is no induction valve, (in the common meaning of the term,) the water cannot be driven back through it, (when out of order,) into the supply cistern, which I have often observed to be the case in common pumps. The power lost by the up-stroke of the pump is not more than a 200th part of that of the engine, which was considered as of less importance than the insuring of a regular supply of water to the boiler. I do not, however, consider it as a perfect machine, and I notice it merely that your committee may give the intention, rather than the plan, their attention.

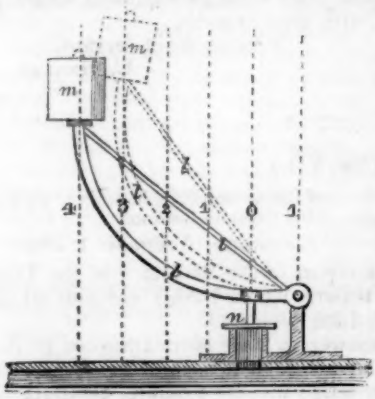
In the application of revolving cocks for feeding high pressure boilers, I consider it as absolutely necessary that the water should be filtered, provided it contains siliceous, or other hard, sandy particles, which are particularly destructive to apparatus of this kind. Revolving cocks were applied by your ingenious countrymen, Mr. Jacob Perkins, and Mr. Joseph Eve; but how far they have succeeded in rendering their action preferable to force pumps, I have not had the opportunity of learning.

With respect to obtaining increased security from safety valves, I have two or three plans to submit to the consideration of your committee; annexed is a sketch of one of them.



At *a* is intended to be represented a metallic cap, inverted, whose edge is to be nicely turned, so that its periphery may lie in a true horizontal plane, upon a flat hard steel plate, *b*, fixed to the top of the boiler, *c*; the hole at *d*, is made much larger than usual, (to allow the steam to escape quicker,) requiring, in consequence, a heavier weight, *e*, or a lever, *f*, of greater effect to keep the cup to its seat; *g* is a box to receive, and *h* a pipe connected thereto, to carry off the steam that escapes from the boiler. At *i* is a sliding loop or stop, which is fixible at pleasure on any part of the lever by means of a finger-and-thumb screw; the intention of this is to limit the range of the weight *e*, which is suspended to the axle of a little wheel, *k*, that runs upon the upper edge of the lever. This part of the apparatus was devised from noticing a recommendation of Mr. Tredgold, in his excellent treatise on the steam engine; that safety valves should be so constructed as to be relieved of a part of their load, when they are raised by the steam. That intelligent writer having, however, omitted to give any plan, I submit the foregoing to the consideration of your committee, as being calculated to attain that object by simple and efficient means.

Although this valve may be found advantageous as applied to land, or fixed, engines, your committee will immediately perceive its ineligibility for steam-boats, the lurching of which would prevent the weight from acting always in the manner desired. To obviate this objection, as applied to steam vessels, I propose to relieve the valve of a portion of its load by another arrangement, which I suggested two or three years ago, in the Register of Arts. This mode is represented in the annexed diagram, in which, in lieu of a *horizontal* lever, I have an *inclined* one, *l*, fixing the weight, *m*, on the upright portion of it; so that when the valve is lifted any given height, by the pressure of the steam, the weight is in effect brought nearer to the fulcrum of the lever; as the forces act perpendicularly, in the manner represented by the dotted lines in the figure, and a lever having the power of 4, is reduced to a power of 3 by a very slight motion of the cover of the valve; the several parts of which differ only from the first, in being an inversion of their order; *n*, in this case, being the steel plate, and *o* a piece of tube, whose edge is accurately turned, so as to touch the plate throughout the line of its periphery. Practical men will decide which plan merits the preference, either as respects the utility, or the facility of construction. There should, of course, be a stop, to limit the range of the lever.



On the same page of the Register wherein I suggested the last mentioned plan, I proposed, as an *additional* security to boilers, that when the mercury should be forced out of the gauge by the undue pressure of the steam, it should be received into a vessel suspended to the power end of a long lever of the first class, whose other end, (near to the fulcrum,) should be made to lift a safety valve from its seat, loaded with a weight greater than that which ejected the mercury from the tube. By this arrangement the valve would be kept open until matters were arranged, and security to persons and machinery be instantly afforded. If the principle of this proposition be approved of by your committee, I need not point out to them the means of carrying it into execution.

As the various suggestions published in the scientific journals of Europe and America, will, of course, undergo the investigation of your committee, it would be supererogatory in me to notice them particularly; but I will just mention that the fusible plug, and Sockl's valve, seem to me to deserve their consideration; and were I not

fearful of rendering my letter too tedious to be read, I could mention various other plans to effect the same object that have occurred to my mind; I will, therefore, here conclude my already too long letter, with expressing the hope that *something, however little*, may be gleaned from it, and my anxious desire that the committee may rather be induced to inquire whether effective boilers cannot be made, which would injure no person or thing, if an explosion should take place, than into modes of preventing the bursting of boilers that scatter death and destruction around them when an explosion occurs.

I am, with great respect,

Your obedient servant,

L. Hebert.

(No. XII.)

Letter from Thos. W. Bakewell, Esq., of Cincinnati, Ohio, in reply to the Circular of the Committee of the Franklin Institute.

Cincinnati, November 1, 1830.

I now enclose a copy of my report to the Secretary of the Treasury, on explosions of steam boilers, which I think contains all the essential information of which I am possessed.

Please receive this as my answer to your communication on that subject, which would have been attended to sooner, but for private engagements, and a latent conviction that the Franklin Institute did not stand in need of my services.

I am,

Very respectfully, yours,

Thos. W. Bakewell.

FOR THE FRANKLIN INSTITUTE.

Copy of a Report to the Secretary of the Treasury.

Sir,—A copy of your letter to Morgan Neville, Esq., of this place, together with a list of interrogatories, relating to the explosions of steam boilers, have been placed in my hands by that gentleman, with a request that I should give such information in reply to them as I may possess.

The interrogatories appear to be addressed more particularly to those who may have been present at an explosion of a steam boiler, and to relate to the circumstances growing out of a disaster of that kind. As I have never been present at an explosion, I shall proceed to the subject apart from the order in which the interrogatories are made. It is desirable that a statement of this nature should be composed of as much of fact, and as little of theory, as possible; and the subject has consequently engrossed my attention for several months, in obtaining such information as might enable me to present a full narration of facts attending many of the explosions on the western waters; but in this I have been disappointed, owing to the confused

and contradictory statements of the parties present. I am, therefore, compelled to rely, in a great measure, on the impression made on my mind, by the several accidents, at the times, respectively, when they occurred, with the consequent risk of my report being biassed by my own peculiar views.

I have been, theoretically and practically, acquainted with the steam engine, both in the making and using of that agent, for nearly twenty years. My experience in its use has consisted chiefly in its application to boats, and I believe no alteration or improvement, either real or pretended, during that period, has escaped my notice.

I have every reason to believe, that most of the explosions of boilers arise from the simple fact of the steam being urged to a density beyond the strength of the boiler, unconnected with the production of hydrogen gas, or any sudden chemical change in the constituent parts of water. But that explosions have occurred, when the steam has not been above the usual working pressure, and under circumstances which cannot be explained, but by admitting the rapid production of some kind of gas, or steam, in quantities too great to be carried off by the safety valve. The *immediate* cause of this last species of explosions, cannot, with certainty, be known. It is, however, generally agreed, that it never takes place unless from a want of a sufficient supply of water, certain parts of the boiler or flues become red hot. The steam, also, in this case, will sometimes be so much heated as to set fire to the deck, or any combustible substance in contact with the boilers; and the steam admitted to the engine in this state, will burn the hempen packing and wood contiguous to the engine. To the above effects I have been an eye witness, but no explosion ensued. And here I would observe, that although there exists a difference of opinion, as to the proximate cause of explosion in a boiler thus circumstanced, it is generally agreed, that to inject water, before the fire shall have been extinguished a sufficient length of time for the boiler to cool, is the most dangerous step that can be taken.

The two opinions as to the cause of explosions, by injecting additional water, or by agitating that already in the boiler, in this heated state, are first, the oxidation of the hot iron, and the consequent disengagement of hydrogen gas, which, by its subsequent combustion, creates the explosion of the boiler. The other opinion is, that the rapid production of steam, is of itself sufficient to account for the effect, and that if hydrogen gas be generated, it acts, by its expansive force, as a gas, in addition to the steam; and in corroboration of this view of the case, it is alleged, that copper boilers have exploded under circumstances which might well have been attributed to the combustion of hydrogen gas, had not the material of which they were made precluded the idea.

The only case on the western waters which we are compelled to refer to one, or other, of the last two mentioned causes, is that of the steam-boat Grampus, whose boilers, six in number, cylindrical, 38 inches diameter, with two 14 inch flues in each, exploded simultaneously, about daylight in the morning; and after it had been dis-

covered that they contained very little water, and a plentiful supply was suddenly thrown in. It is said, but not well established, that a smoke, and smell of burning, was perceived before the explosion.

Several explosions have taken place by suffering earthy sediment to accumulate on the bottom of the boiler. This sediment will, in time, become heated, or baked, to dryness, and the contiguous metal attain a red heat; when thus weakened the metal is incapable of sustaining the ordinary pressure of steam. The late accident on board the steam-boat Caledonia, was from this cause. But the most frequent cause of explosion has been from the direct pressure of the steam, without recurring to others, more remote or occult.

Owing to explosions having taken place after the engine has made a few strokes, the idea has obtained, that the steam was not at the highest point at the instant of explosion. Moreover, it is contended that explosions have taken place at starting, when the steam was certainly not higher than it had frequently been.

It will be observed, however, that when a boat is *about to start*, it is usual to prevent the further escape of steam through the safety valve, and to urge the fires; and that owing to the gradual and slow motion of the engine for some time, (say half a minute,) after starting, the steam is generating faster in the boiler than is demanded by the engine, and that the point of time, when the steam is at the highest pitch, is more frequently over half a minute, than under that period of time.

It is known, also, that a boiler, or iron shaft, or other piece of machinery, will give way by a continued strain, which strain shall be less in degree than would be sustained for a short time without injury; and that it by no means follows, when a boiler explodes by excess of steam, that it never had previously been subjected to an equal pressure.

I am not aware of any instance where two successive and distinct explosive efforts have been detected, as alluded to in your last query. This, however, was stated to have been the fact in the explosion at Bowen's mill, at Pittsburg, in 1821. I had satisfied myself by personal investigation at Pittsburg, that this report was erroneous, and in a description of that disaster, lately received from Mr. Bowen, the idea is again refuted. The boiler gave way at the bottom, over the fire, and was known to contain sufficient earthy matter, to warrant the conclusion that the baked sediment was the immediate cause of the explosion. The usual source for the supply of water, had been interrupted, and the boiler supplied, for three days previous, with water from a muddy well.

An explosion took place, about eighteen months ago, in M'Mickle's mill at Pittsburg, under circumstances so similar to that at Bowen's, that it is fair to attribute them both to similar causes. It was known in both the above cases that there was no deficiency of water. By the aid of a few figures, (not necessary to introduce here,) it would be easy to show that the steam alone was much more than adequate to the effect, or the projecting of the boilers from the mills.

As an instance of the difficulty of arriving at the truth, in occurrences of this kind, allow me to state, that in the year 1827, the steam-boat Union, Capt. Clark, when a few miles below this place, burst off one of the cast iron ends of her cylindrical boilers, 36 inches diameter. The after end, which flew off, passed, together with water and steam, through the cabin, tearing down all the light partitions which intervened, and laying open the cabin by parting its sides from the deck above. The boiler, by the unbalanced pressure, or reaction, after the end flew off, was projected over the bows of the boat into the river.

The boat was shoving off from the shore at the time, and about to start, but as far as I can learn, the wheels had not made a revolution.

I saw the captain and some of the crew soon after the accident, who assured me there was plenty of water, for he, (the captain,) saw the gauge cocks tried two minutes before the explosion. But at a subsequent period, and after the captain had conversed with some of those who contend that a boiler cannot burst with "fair play," as it is termed, he became of the opinion, that the cause was a deficiency of water, and the flue being red hot; and did not doubt of its being collapsed, and that the formation and combustion of inflammable gas, or some other hidden and uncontrollable circumstance, produced the explosion.

The history of the case rested on these grounds until the boiler was recovered from the river, when it was seen that the flue and interior of the boiler were perfect, and exhibited no signs of having been heated red hot. A thin whitish coating of earthy (probably calcareous,) matter was on the upper part of the flue, which some, at first sight, supposed to indicate its having been heated; but this appearance is observable on all flues after the water is drawn off from the boilers, at the same low stage of the river. The flue in this boiler did not pass through the end which was forced off, but turned down by an elbow, short of the end, to receive the heated air and smoke from the furnace below.

The steam was usually worked at about 80 lbs. to the inch, and at the time of the accident the Union was in contest with another boat.

The principal reason of explosions having been more frequent of late, on the western waters, than formerly, is simply because the engineers work the steam higher; and this they are enabled to do with the same boilers by cutting off the steam shorter, not only with cams suited to that end, but by "pinching" the steam at the throttle valve.

This mode of proceeding, together with the prevailing notion, as before stated, that boilers do not, or will not, burst by "fair play," that is, by the pressure of the steam alone, with plenty of water, has been the main source of the recent disasters.

The success which has attended the British act of parliament, predicated on the most direct and obvious view of the case, to wit: that boilers burst because the steam is too strong, or the boiler too

weak, is deserving of our serious consideration. That act provides that every boiler shall be tried by hydrostatic pressure to bear three times the strain, which shall be indicated by an extra certified safety valve.

The details of the examination inducing this act may be found in "Partington, on the Steam Engine," London Ed. of 1822. I conceive that this very severe test secures the boilers from explosion, not only from the pressure of the steam *with* a sufficient supply of water, but in all ordinary cases *without* a proper supply; for with this required extra strength, the flues of most boilers would sustain the working pressure at a red heat, and the boilers of the English boats are exempt from one of the sources of accidents with us, viz. the gradual wasting and weakening of that part of the bottom of the boiler, which in many of our boats is exposed to the action of the fire, and kept in a red hot state by baked and dry earthy sediment over it.

The bottoms of their boilers, where the sediment settles in injurious quantities, are not exposed to the action of the fire any more than those of most of our eastern boats; and a very slight test of strength, beyond that allowed by the safety valve of the "Chief Justice Marshal," would have shown that the lower part of her boilers, or large flues, was insufficient to stand the fair pressure of the steam, though not exposed to the fire.

If a test, equally severe with that of the British act, were required in this country, nearly all our boats, both eastern and western, would be rendered useless; and it therefore remains to be considered, to what degree it may be imposed, without being oppressive, in the present condition of our steam navigation.

Permit me to suggest, that all boilers of boats, used for the conveyance of passengers, be proven at least once in twelve months by hydrostatic pressure, to be capable of sustaining *double* the pressure that should be indicated by an extra safety valve; which said extra safety valve should be under the sole control of the master of the vessel, and he be under bond and oath that no greater pressure of steam should ever be carried than shown by the lifting of said extra safety valve. That surveyors be appointed at convenient ports to see to the proper and faithful carrying into effect of this measure, who should grant their certificates accordingly.

Every boat has, or may have, at a small expense, the means within herself of applying this required test, by attaching a temporary lever to her forcing pump, to be worked by hand, or a small one provided for the purpose.

This test might be relied on for preventing explosions from the usual cause, (excess of steam,) and also, in most cases, where the accident takes place from the metal being weakened by heating.

I know of no legislative enactment that can provide against a deficiency of water, or an accumulation of mud, or saline deposit, in the boiler.

The contrivance called a "tell-tale," designed to give notice when the water gets too low, and sometimes made self-acting, so as to supply the boiler, is with reason objected to by practical men as uncer-

tain in its own operation, and tending to lull the engineer into carelessness and false security, and whenever the boat should have motion from a sea, would be utterly useless. It is true that the constant turning of the gauge cocks is an irksome office, and it is apt, on this account, to be neglected. If some more easy and agreeable mode could be adopted, of making the engine tender an *eye witness* to the height of the water in the boiler, it would go far towards removing danger from this source. Tubes of glass have been tried in Pittsburg and in this place, the lower ends of which communicated with the water, and the upper ends with the steam, which answered the purpose completely so long as the tubes remained entire; but the manufacturers have not been able to anneal any tubes sufficiently to stand long in that situation.

I am informed that glass in the form of tubes when of the required thickness to sustain the mechanical pressure to which they are exposed, will not stand the inequalities of temperature at the inner and outer surfaces, but that there is no difficulty in making a *plate* of glass stand; and I have, in consequence, ordered some plates for this purpose, which I intend to insert in the sides of a square cast iron pipe, attached to the end or side of the boiler.

Of High and Low Pressure Engines.

The comparative merits of these two kinds of engines, both as regards safety and efficiency, have become somewhat of a party question, and great caution is required in receiving the opinions of any on this point. It may, therefore, be expected that I should be prejudiced in favour of the one or the other, and there is no reason to hope for impartiality in the present notice of them, except so far as the opinions of others, (with respect to myself,) and the using of both kinds of engines, may give grounds for supposed impartiality. Both these circumstances exist in favour of an unprejudiced report of them.

The proportional extra strength of *all* boilers, over that of the steam intended to be carried, may generally be assumed as equal, and the liability to explode as equal, but the disastrous consequences in the event of an explosion may be estimated to be in proportion to the height or strength of the steam at the time of explosion. Hence it must be inferred that the balance of safety is on the side of the low pressure engine, and I believe the experience of the last ten years will bear out this conclusion; not so much by the greater number of accidents with high pressure engines, as by the greater destruction when they do happen.

To engineers "high and low pressure" are indefinite terms. Engines which condense the steam, (as distinguished from those which suffer it to escape in the air,) work steam at all the various pressures from 5 lbs. to 100 lbs., on the square inch, and as good a vacuum may be obtained in the one case as in the other, contrary to popular opinion, and that expressed in several works on the subject. The same quantity of caloric, (and of steam by weight,) will pass to, and be overcome by, the condensing apparatus, with the same furnace

and boilers, whether the steam operate through a large cylinder at five pounds, or through a small one at 100 lbs. to the inch; but the *nett* gain by the vacuum, on every square inch of the piston, (say 10 lbs.) is the same in both, so that the smaller the cylinder, and the higher the steam worked, the less does the vacuum become an object, and is consequently, in most cases, relinquished altogether, and then the engine is called "high pressure."

Very respectfully, &c.

(Signed,) THOS. W. BAKEWELL.

Cincinnati, 1st November, 1830.

(No. XIII.*)

Cincinnati, January 19, 1831.

DEAR SIR,—Your favour of 3d inst. is to hand. The only additional experiment to those enumerated, which I think desirable, is in connexion with No. 7. This experiment, (No. 7,) is to "ascertain whether any other gases than steam are produced in the boiler;" but it is of the greatest importance to know whether any other gases thus supposed to be produced will explode, or ignite, in the boiler producing them.

If a boiler be filled with water, (or nearly so,) and the steam suffered to escape at the top, as is the case with an engine boiler, it may be presumed that all the atmospheric air is expelled from the boiler, and the water, before it should become so low as to expose the hot metallic surface to the steam. If the water, (or steam,) be decomposed by the heated metal, and the combustion of the hydrogen gas can, at any subsequent period, take place, while in the boiler, what prevents the combustion in small portions as soon as it is formed, and while in contact with the heated metal, (thus preventing an accumulation of it;) or what degree of accumulation should determine an explosion? The opinion I hazard on this point is, that if hydrogen gas be produced in a boiler, *circumstanced as above*, it would only be similar to a gas generator, and that the gas would not ignite until it had access to the atmosphere, and that the gas issuing from an orifice might ignite, but would not produce the combustion of that within. I know it has been stated, (hypothetically,) that a part only of the oxygen is taken by the metal, and a part left in a state of mixture with the hydrogen, which, *in due time*, causes the explosion.

I am, very respectfully,

Yours,

THOS. W. BAKEWELL.

[TO BE CONTINUED.]

* In reply to a letter stating the course of experiments which the Committee of the Institute were about to undertake.

TO THE EDITOR OF THE JOURNAL OF THE FRANKLIN INSTITUTE.

Observations on the importance of Meteorological Observations, particularly as regards the Dew Point: and also on the several fluctuations of the Barometer, by JAMES P. ESPY, *Professor of Languages.*

IN a previous essay I endeavoured to awaken the attention of scientific men to the department of meteorology. I ask again why is it that this highly interesting and useful branch of human knowledge makes such slow advances, whilst almost every other department of physical science is receiving important additions every year?

The labours of chemists have made us acquainted with the elements of the atmosphere which surrounds us, and the illustrious philosopher of Manchester, has enabled us at any moment, with the simple aid of a common thermometer, and a tumbler of cold water, to tell with absolute certainty, not only the number of grains* of aqueous vapour contained in a cubic foot of air, but the whole weight of the aqueous atmosphere to its very surface; and yet with this powerful instrument in our hands, meteorology languishes; we cannot yet predict the weather forty-eight hours in advance—the very cause of rain is unknown—and the "*Dew Point*" is published in only one journal in the United States.

Let learned and scientific societies answer to themselves and to the interests of mankind for this egregious neglect. Do philosophers think that no more discoveries can be made on this subject? and that the weather never can be predicted?

If indeed we were obliged to depend upon those imperfect hygrometers in use before Dalton's discovery, there would be some reason to despair of ever being able to predict the weather; but when we consider that the quantity of vapour in the atmosphere is the only constituent which changes its proportion, varying from $\frac{10}{1000}$ of the whole atmosphere when the dew point is 13 Fah. to $\frac{88}{1000}$ when the dew point is 76, (indeed the dew point sometimes sinks lower than 13 Fah. but I have never seen it higher in Philadelphia than 76,) and when we consider that the weather depends so much upon the quantity of vapour in the atmosphere, it becomes highly probable, that cotemporaneous observations, made on the quantity of vapour alone, in different parts of our widely extended country, would solve the problem of the weather, which has hitherto eluded the researches of the most patient observers.

My object in this communication is to endeavour to awaken in your readers an interest in this subject by communicating a few facts of my own observation, and correcting some prevailing errors which vitiate all systems founded upon them.

It is, I believe, universally admitted, that whenever the air over a particular region begins to be expanded by heat more rapidly than the air of surrounding regions, a flow of air immediately takes place towards the hotter part.† Now this is not the fact—for the

* See table at the end of this essay.

† See review of Mr. Daniell, American Quarterly, for March, 1828.

air, in becoming hotter, increases in elasticity, and presses harder in all directions than it did before, upwards, and downwards, and laterally; and moves the surrounding air outwards as well as upwards until the equilibrium is restored. This increased elasticity, or pressure of the air, is indicated by the barometer after sunrise in all climates, particularly in the torrid zone, where its semidiurnal fluctuations are so regular that its point of elevation almost indicates the hour of the day. It rises regularly from sunrise till 10 o'clock, about which time the heat is increasing most rapidly; it then falls till five, about which time the rapidity of refrigeration is the greatest, and, of course, the pressure least; after which it rises by reciprocation till about ten, and then falls till sunrise, for the same reason. These fluctuations may be detected in this latitude by taking the mean of a great many observations, recorded at 9 or 10 A. M., and 5 P. M. In the city of Philadelphia, 45 feet above the sea, very careful observations have been made on the barometer since the 1st of April of the present year.

The barometric mean for April, between 9 and 10 o'clock, A. M. was 30.026 inches, and at 5 P. M. 29.968. For May, at the same hours, 30.04 inches, and 30.00 inches. For June, at the same hours, it was 30.145 and 30.111, and for July 30.10 and 30.06. For each of these months, then, the barometer, at a mean, has been about four one-hundredths of an inch higher between 9 and 10 o'clock in the morning, than at 5 in the afternoon. No doubt the fluctuations by reciprocation could be detected by observing the hours in which they take place. This I intend to do, before you receive my next communication.

The general principle which I wish to state as the cause of the semidiurnal fluctuations of the barometer, is that a sudden increase of temperature in the air will cause an increased pressure on the barometer, and vice versa, a sudden diminution of temperature will cause a fall of the barometer; but it must be understood that these effects on the barometer will be observed at the moment of the change of temperature, before the quantity of air over the observer has time to be increased or diminished by flowing *on* in the upper regions in the case of refrigeration below, or flowing *off*, in case of expansion, by increased heat.

To illustrate my idea by an example: Suppose at sunrise the temperature of the air over the whole island of Great Britain to be 32° Fah., and suppose the temperature to increase to 42 by 10 o'clock, of all the air covering the island, to the height of 96 feet. Now, as the experiments of many philosophers prove that the bulk of air, at the temperature of 32, is increased $\frac{1}{480}$ for every degree of Fah., therefore, by an increase of 10 degrees, this lower stratum of air, 96 feet high, will be increased in bulk $\frac{1}{48}$ of the whole; and if the air around Great Britain, to a great extent, expands as fast as the air over the island, then will the pressure of air inwards, from the ocean to the island, be equal to the pressure outwards, and the whole atmosphere over the island will be thrust upwards 2 feet by the expansion of the lower stratum of air 96 feet thick.

But if the lower stratum of air over the ocean does not increase in

temperature, after sunrise, as fast as that over land, then will the effect of some part of the greater expansion over land be, to thrust the air out laterally from the land to the ocean; but in either case, the barometer will rise while this process is going on. This rise, moreover, will be in proportion to the rapidity of the increase of temperature; for if this lower stratum of air could be supposed to increase in temperature instantly 10 degrees, then would there be a rise of the barometer for a moment nearly $\frac{1}{48}$ of 30 inches; for the inertia of the air itself, would, for the first moment after the increase of temperature, afford nearly the same resistance to the motion of expansion, as if it were confined by an inexpansible vessel.

A remarkable proof of the great resistance inertia makes to sudden motion, was given some years ago in Silliman's Journal. A quantity of fulminating powder lying on a table and covered with a bell glass, accidentally exploded—the bell glass was merely lifted up a short distance, and broke in falling, but a hole was made through the table. The inertia of the powder, together with the atmospheric air, was so great as to react on the table with such force as to pierce a hole through it.

If I have been so fortunate as to render myself intelligible in showing the cause of the rise in the barometer by an increase of heat, I flatter myself that any one may easily perceive the cause of depression in the afternoon, and the two remaining fluctuations by reciprocation during the night, without any further illustration from me. I will, however, before I pass to another subject, answer one objection which may be proposed to the explanation here given. It may be asked if increase of heat in the air is the cause of a rise of the barometer, why does not the barometer continue to rise till the hottest part of the day? I answer, this would be the case if the air were confined in an inextensible vessel. This, however, is not the case, but the whole column of atmosphere above the expanded part, is raised, and the greatest resistance it makes to raising is at the moment when it is thrust upwards with the greatest velocity; that is, not when the heat is greatest, but when it is most rapidly increasing, which is known to be about 9 or 10 o'clock; about half way between the time when it begins to increase, at sunrise, and ceases to increase, at the hottest part of the day.

If I have been successful in establishing my own theory of the semi-diurnal oscillations of the barometer, I need not stop to refute Professor Leslie's theory of the sea breezes being the cause, though it is known that these oscillations take place in the midst of the great Pacific ocean. Nor Mr. Daniell's theory, which makes these oscillations depend upon the polar currents, and whose theory makes it necessary that on the same longitude, even at the autumnal and vernal equinox, the barometer should fall in high latitudes at the same moment it rises in low. Nor M. Ramond's theory, though quoted with approbation by M. P. E. Morin, which makes these oscillations depend on the increased and diminished quantity of air over our heads; that is, that more than a mean quantity is over our heads at 9, A. M. and less than a mean quantity at 5, P. M., which is caused by the surface of the atmosphere being elevated above its mean

height at noon, and depressed below its mean height at midnight, and of course at noon it runs off, and at midnight it runs on, from the east and the west of those longitudes.

It appears at first sight that some effect should be anticipated from this principle. It is worth a moment's reflection: let us subject it to actual calculation.

It is known that at the height of a few hundred feet, the air changes very little in temperature during 24 hours when it is clear, for the rays of the sun pass through clear air without heating it much, and it is chiefly by contact with the surface of the earth that the lower stratum of air becomes heated during the day, and cooled during the night. Even in this lower stratum, the difference of temperature between day and night does not often exceed sixteen degrees. Let us, however, put an extreme case, and suppose that the difference of temperature of the lower stratum of air 500 feet high is 20 degrees; the expansion due to this elevation of temperature in the day above the night, is about 40 feet. If, now, we suppose the whole of the atmosphere above this to vary in temperature daily one degree, the elevation of the atmospheric column due to this increase of temperature in the day, will be $\frac{1}{480}$ of the whole, if we suppose the mean temperature of the air to be 32. Now $\frac{1}{480}$ of 40 miles, the height of the atmosphere, is 440 feet; add to this 40 feet, the expansion of the lower stratum of air, as above, and the whole elevation of the atmosphere, at the hottest part of the day, above the coolest, is 480 feet. Now let us see with what velocity the upper stratum of air will move from the greatest elevation at noon, towards the greatest depression at sunrise. We have then an inclined plane 480 feet high, one quarter of the circumference of the atmosphere in length; that is, about 33,000,000 feet, or 68,800 times its height. Now, by mechanics, a body free from friction would run down such an inclined plane $\frac{1}{68800}$ part of $16\frac{1}{12}$ feet in one second of time, and as the spaces passed over down inclined planes are as the squares of the times, in six hours, even if the height of this inclined plane did not diminish, which it does, the space descended along the plane would only be 109175 feet, which is about $\frac{1}{302}$ of the whole elevation; that is about 19 inches of atmospheric air at a mean density; a quantity too small to be appreciated in the barometric fluctuations. Many other objections might be brought forward against this theory of M. Ramond, but I forbear to state them now.

The next prevailing error I shall notice is, that "mountains attract clouds." See Edinburgh Encyclopedia, article Hygrometry; and Library of Useful Knowledge, article Physical Geography. Now that clouds frequently appear near the tops, and on the sides of mountains, when they appear no where else, is a well known fact. Let us see how this can be accounted for on established principles, without supposing any mysterious attraction for which no reason has ever been assigned.

Mr. Ivory says, "if a mass of air were suddenly reduced to half its bulk, the heat evolved would be 90°"—consequently if it were suddenly expanded to twice the space, it would lose 90° of tempera-

ture. (Phil. Mag. Feb. 1827.) Suppose, now, to put an extreme case, a current of air at the temperature of ninety, completely saturated with moisture—that is, having its dew point at 90°, should meet a mountain, and blow up its side; by ascending, it would expand, and by expanding it would become colder one degree for every $\frac{1}{90}$th of expansion; let us see how much vapour would be condensed from a given quantity of air when it has ascended so high as to occupy double the space, and when the temperature is zero. By the table given at the end of this article, a cubic inch of air, saturated with vapour, at the temperature of 90°, contains .00836386 grains of aqueous vapour, and at the temperature of zero, 2 cubic inches, saturated, contain .00089914 grains. Now the former quantity is more than nine times the latter; therefore more than $\frac{8}{9}$ of the aqueous vapour which the air contained at the foot of the hill, will be deposited in rain before it rises so high as to occupy double the space. This calculation is on the supposition that the ascent is so rapid that it takes a temperature due to sudden expansion. If it ascends very gradually it will be shown presently that its temperature will not sink so low by about 18 degrees, for the mean temperature of the air at that height is about 72 degrees, and not 90° lower than that at the surface of the earth. But even at this temperature the quantity of moisture could not be quite one-fifth in the upper station of what it is in the lower. Thus have I demonstrated that nothing is necessary to produce clouds and rain on the sides of mountains, but an ascending current, whose dew point, at the base, is near the temperature of the air. That such a current upward actually takes place, every one who has been on the side of a mountain many times can testify, for he has felt the air blow upwards.

The principle here developed may also serve to account for the fact that the quantity of rain on the sides of mountains is greater than on the plain below. For notwithstanding M. Leslie has given currency to an opinion that more rain falls in low situations than high, the reverse is a well established fact. Indeed the whole economy of nature requires it, and we might be pretty certain of the fact *à priori*.

"In the neighbourhood of Kinfaun's castle," (says the Edinburgh Encyclopædia, article Meteorology,) "a rain guage is placed on the summit of a hill, 600 feet above the level of the sea, and another in a garden at the base of the hill, about 20 feet above the sea. The following is the mean result of the two for five years: from 1814 till 1818, hill guage $38\frac{84}{100}$ inches, garden guage $25\ \frac{66}{100}$ inches.

"No part of the above difference can be ascribed to any thing in the situation of the garden guage, but its being lower down. It is fully exposed to the rain, and as the best proof of this, its average amount for six years is about two inches greater than that of another, situated in an open country, at the distance of about a mile."

Indeed there is strong reason to believe that even at a distance from mountains there is more rain falls at some distance above the surface than at the surface itself. This is certainly the case sometimes, for I have known a large shower to take place when the lower

stratum of air was more than 20 degrees above the dew point, and consequently the drops of rain falling through this heated air would certainly evaporate some; and as an absolute proof of this, I have known the dew point to rise two degrees during a shower of half an hour.

It is true, the reverse of this sometimes takes place, for the drops of rain coming from a great height enter the region below with a temperature greatly below the dew point, and of course increase in size; whether this latter effect is equal to the former cannot be determined from theory in its present imperfect state.

However this may be, it is certain it frequently rains above when it does not rain below. In the middle of every dense cloud there is a considerable rain. The upper part of the cloud is in air cold enough to condense the vapour, and the lower is in air warm enough to evaporate it, and the middle frequently consists of pretty large drops of rain, which fall with velocity enough to give an impulse to the air, which is felt at the surface of the earth, for the cloud, (that is, the rain,) is the cause of the accompanying wind, and not the wind of the rain.

In meteorology it is not always easy to distinguish of two concomitant phenomena, which is the cause and which the effect. For example, it has, universally, so far as I know, been believed that the very great flood of rain which frequently succeeds immediately, or accompanies, a very sharp clap of thunder, is caused in some way by electricity. In Walsh's Notices of Brazil, vol. ii. p. 92, a thunder storm is explained on this supposition.

"It becomes quite dark in mid-day, except when some lurid blaze enveloped us, which was accompanied by a sheet of water, which fell on us like a cataract, and almost beat us to the ground. The explosion of sound immediately followed the flash.—If I could have divested myself of the alarm which the immediate proximity of such awful danger excited, I should have been delighted to contemplate the chemistry of nature on her grand scale. I remember with what pleasure I had seen Sir H. Davy produce water from the combustion of hydrogen and oxygen. Here it was generated in an instant from the same cause, and I was standing in the midst of the combustion, and admitted, as it were, into the very interior of nature's great laboratory."

Again, in the article Meteorology, mentioned above, "That electricity is a frequent and powerful agent in the formation of rain is extremely probable. The well known fact that the rain which accompanies a thunder storm is more copious than in any other circumstances, is evidence sufficient that it is frequently modified or increased by the influence of electricity."

I once thought with the writer of this last quotation; but when I consider that the immense flood of rain which frequently accompanies thunder succeeds it so immediately that the drops could not have time to fall many feet, much less from the clouds; and that Mr. Walsh's idea cannot be correct, for there are no hydrogen and oxygen uncombined in the atmosphere to produce combustion; and if

there were, the result would be aqueous vapour, not water; I was compelled to look for some other explanation of this phenomena. The true solution of the difficulty, I think, will be found in considering the rain as the cause, and the lightning as the effect.

When a large quantity of rain is discharged from the clouds at once, and when the lower drops approximate very near to the earth, whilst the upper drops reach the clouds, they form an immense *discharger* for nature's great battery, and the lightning is discharged just as the lower drops of this sudden deluge are about to fall to the earth; but as the great mass of rain generally succeeds the electrical discharge, it is not wonderful that the cause should have been considered the effect, and the effect the cause.

The utility of lightning in the economy of nature, is yet to be discovered. That it, and the means of predicting the state of the weather will be discovered, at no very distant day, I have little doubt. In the mean time, nature must be patiently interrogated, and faithful answers will certainly be returned.

I come now to speak of an error of great magnitude, and of universal prevalence, I believe, in this country. It was first conceived by Professor Dwight, and it has lately been assumed by Professor Mitchell as the very foundation of a laborious theory, in which he endeavours to explain many meteorological phenomena, particularly why the mean annual temperature of our continent east of the Rocky mountains, is lower than that of Europe on the same latitude. The error I allude to is, that the air in large quantities sometimes descends from the upper regions to the lower, and brings with it a great and sudden depression of the thermometer; and that universally when the wind blows across mountains, in descending down the side to the leeward, it brings down cold weather with it. It seems to be taken for granted by these theorists, that because the air is at a lower temperature in the higher regions than in the lower, if this upper air were brought down by any means, it would therefore be cold; forgetting another principle with which they are familiar, that air suddenly condensed by pressure becomes hotter. Now whether a portion of air suddenly brought down from the upper regions and subjected to a greater atmospheric pressure below, would be hotter or colder than the air at the surface of the earth, can only be ascertained by observation and experiment.

In the July number of the Philosophical Magazine, 1825, Mr. Ivory says, that Mr. Ramond had collected forty-two different measurements for the purpose of ascertaining the decrease of temperature in ascending the atmosphere. The greatest altitude in these is Gay Lussac's ascent, nearly 7600 yards; and in this particular case the height for one degree Fah. is 105 yards. The average of the whole, however, is 100 yards to a degree.

In the ascent of Gay Lussac, the thermometer fell from 55.4° to 17.1° below zero, that is 72.5° Fah.; at the same time the elasticity or weight of the air was reduced from 1 to 0.432, and the density to one-half very exactly. Now it is known by experiment, as was mentioned above, that air suddenly reduced by pressure to one-half the space

acquires 90° of temperature; that is, if Gay Lussac could have transported some of the air from the height to which he ascended where the density was only one-half to the surface of the earth, where its density would be doubled, its temperature, if it neither lost nor acquired caloric, would be increased 90°, and so be 17.5° warmer than the air at the surface of the earth. In like manner it may be shown that if air be brought down from any other elevation, its increasing density will constantly keep its temperature above that of the air into which it is successively introduced, and the greater the elevation from which it is brought, the higher will be the temperature above the stationary air below.

Indeed it is easy to show that if the reverse of this is the fact, the equilibrium of the atmosphere could not be preserved; for if a current once commenced downwards, it would always continue to flow; for the descending column of air being, by supposition, colder than the surrounding air, would always be heavier, and it would consequently rush downwards from above, and outwards at the base, with a fury proportional to the supposed difference of temperature. Now as no such vortex exists in the atmosphere, we might, with great certainty, infer, á priori, that the air in the upper regions is not colder than at the surface of the earth it would become by being suddenly expanded to the same degree of rarefaction. It is known that a pound of steam, if the space it occupies is saturated, contains the same quantity of caloric at all temperatures. From this, Mr. Dalton inferred that every pound of atmospheric air contained the same quantity of caloric at all elevations; in this, however, the philosopher of Manchester was wrong, as was afterwards demonstrated by Mr. Ivory. Indeed it follows from the example given above, that air, rarified suddenly at the earth's surface, is only one degree in four colder than air of the same elasticity in the atmosphere, for 90 is nearly one-fourth greater than 72.5. Nor is it at all wonderful that the air in the upper parts of the atmosphere should contain more caloric to the pound, than it does below, for there the vapour is condensed into water to form clouds and drops of rain, and every portion of vapour which is condensed into water, gives out nearly 1000 degrees of caloric. (See Ure's Chem. Dictionary, article Caloric.) Perhaps it may appear inconsistent to some that the vapour should condense among strata of air which contains more caloric than an equal weight of air near the surface of the earth. But if they will recollect, that the vaporic atmosphere is more elastic than atmospheric air, and that, consequently, its density in ascending does not diminish as rapidly as that of atmospheric air, they will see how it should condense above rather than below. For more particular information on this point see Daniell's Meteorological Essays, a work invaluable to the student of this most interesting science.

In fine, on this point, I think it very clear that the air coming across the Rocky mountains cannot, on any theory, cause a diminution of temperature on the eastern side, for if the air becomes very cold by ascending on the western side, for the same reason it be-

comes very warm again by descending on the eastern. Indeed, on the principle demonstrated above, the air in ascending a mountain becomes colder at the top than air of equal elevation over a plane, and yet, in descending on the other side, it must manifestly recover its original temperature when it has descended to the same level.

Before I proceed to another subject, I will answer one more objection which might be urged against my position, that air brought from any height, however great, is not colder than the air into which it is introduced below, but warmer one degree in four. It is known that a summer's shower always cools the air, particularly if accompanied by wind, which is evidently caused by the downward motion of the air in the midst of the shower, for the wind blows outwards from the shower below in all directions, just as it should do on supposition of its descent in the middle of the shower. I answer, this is undoubtedly true, but the descending of the air is not the cause of the cold, but the cold is, in part, the cause of the descending of the air. I say in part, for it is partly caused by the impulse of the drops of rain in falling through the air. But it may be asked, how is the air cooled? The answer is easy. By the rain itself descending from a great height, in many cases much beyond the region of congelation, which in this climate is not much above two miles and a fifth. Being poured out in this cold region it descends into the warm air below, cools it down in some degree, and in cooling it dissipates into vapour: meantime another and another flood succeeds, until finally the air is cooled down almost to the dew point, when the rain which descends from above now reaches the earth, and the whole column of air under the cloud being now cooled many degrees lower than the surrounding air, descends, and in descending spreads out below, and produces a wind in all directions from the rain. It is not, however, to be supposed, that the wind at the surface of the earth will extend to any great distance from the shower; for the great condensation of the column of air under the cloud, and the still greater condensation of vapour in the cloud, will cause the air from the surrounding regions to flow towards the cloud; and this any person who attentively observes the phenomena which attend a summer's shower, may readily discover.

Do mountains then, it may be asked, produce no effect on climate? Yes, certainly; not so much by changing the temperature of the air crossing them, as by causing a diminished flow of air, especially near the surface of the earth, from one climate to another. The Rocky mountains for instance, by the winds which blow from the great Pacific ocean, cause the summers on the eastern side to be warmer, and the winters to be colder, than they would be, if there were no obstacle presented to these prevalent west winds; for it is too well known to need discussion here, that sea breezes render climates subjected to them, more temperate, both in summer and winter. This accounts in the most simple and satisfactory manner, for the mildness of the winters, and the coolness of the summers, on the western coast of Europe, in high latitudes, where the predominating winds

are from the west, when compared to the severity of both winters and summers on the eastern coast of North America on the same latitudes. The ocean being warmer in winter, and cooler in summer, than the continents on the same latitudes, the wind which blows over the Atlantic, when it reaches the shores of Europe, must partake of this mild temperature.

A question here naturally presents itself. Is the cooling effect of the air from the ocean on the coast of Europe as great in the summer, as the warming effect in winter? Or, in other words, is the mean temperature of the year the same in Europe and America? Mr. Leslie answered this question in the affirmative. But it is now ascertained by actual observation that Quebec is as cold as Stockholm, though the latter is nearly 13 degrees of latitude further north; and that Cambridge, U. S., has the same mean annual temperature of London, though the latter is 9 degrees further north. If we examine why the sea changes its temperature for the different seasons less than the land, we shall discover that the difference between its temperature and that of the land is much greater in winter than in summer; and, consequently, the heating effect of the air which blows from it on the land in the winter, will be much greater than the cooling effect in summer.

The reason why the sea is colder than the land in summer is, because the rays of the sun penetrate and heat the water to the depth of many feet, whilst on land their influence is spent in warming the very surface. Some effect also may be produced by the mixing of the water by agitation, and also by evaporation, which may be supposed to be greater at sea than on land. But the whole amount in summer cannot be near so great as in winter, for in the autumn, as soon as the surface of the water begins to be cooled down in the slightest degree, it sinks, by its greater specific gravity, and the warmer water below takes its place, until the whole mass of water, to the very bottom, is cooled; and from this principle alone, the temperature of the sea at the surface will be preserved many degrees above that of land during winter. But this is not all; the specific gravity of the colder sea water at the north being greater than that of the water at the south, will, by hydrostatic pressure, cause a current of water at the bottom of the ocean, from the north towards the south, which must cause a current in the contrary direction at the surface, and all these effects, aided by the Gulf Stream, will keep the temperature of the sea, in high latitudes, many degrees higher than the land. Nor is this merely hypothesis. Baron Humbolt says he has found the temperature of the sea at great depths, even in the torrid zone, as low as 45 Fah. Now this can only be accounted for by supposing an under current from high to low latitudes. For this stratum of cold water lies between a temperature of 80° at the surface, and a temperature below increasing about one degree for every 50 feet of descent, as is abundantly proved by the regularly increasing temperature of mines in their descent. See Cordier on Temperature, &c.

Now if this cold water at the bottom of the ocean were not sup-

plied with water from high latitudes, it certainly would acquire the same temperature as that at the surface.

In the month of December, Jonathan Williams, in passing from the United States to Great Britain, found the temperature of the Gulf Stream 70°, the open ocean 60°, and near the coast of England 48. If we suppose the mean temperature of the whole ocean between Quebec and England to be 56, it will be about 40 degrees warmer than the temperature of Quebec at that season of the year. Again, by a similar investigation, though I have not the exact data before me, I have no doubt it will be found, that the ocean is at most not more than 10 degrees colder during the summer, between those two places, than the mean temperature of Quebec for that time. Thus, upon the whole, it is manifest that the coast of Europe will be more heated by the air from the ocean during winter than it will be cooled during summer. If we had the exact temperature of the ocean in the north, and also in the south, and the mean depth, it would be easy to calculate, from the principles of hydrostatics, how much lower than a mean height the surface of the water in the north stands from its greater specific gravity than that of the south, and the velocity of the current below, towards the south, and of the current above, towards the north, could easily be deduced. In the mean time, I content myself by indicating the principle, and assigning a few reasons for believing that the depression of the surface of the sea in the frigid zone, below that of the torrid, is not less than $27\frac{1}{2}$ feet. The mean temperature of the ocean, within the arctic circle, cannot be far from 32°, for it never rises much above it, and cannot fall much below it, without freezing; for the temperature of the freezing point is stated in the books at 29, though Capt. Parry found it as low as 25, at great depths, on the west side of Greenland, when the temperature of the surface was 32.* At the tropic of Cancer it is about 75°, that is, 43° warmer than the former.

Now water at 32° is about $\frac{1}{22}$ or $\frac{1}{23}$ denser than water at the boiling point 212°: wherefore if the contraction was equable, all the way down from the boiling point to the freezing point, the density of arctic water would be about $\frac{1}{92}$ greater than tropical water. But be-

* In the same latitudes, Capt. Scoresby found on the east side of Greenland, the temperature of the sea gradually increase at great depths, that of the surface being 32. If it should be discovered that the outlet to Baffin's bay is not so deep as Capt. Parry's observation extended, it will afford a satisfactory explanation of the low temperatures of the water at great depths in Baffin's bay. For as it is known that sea water does not follow the law of fresh water, which begins to expand when cooled below 40, but goes on contracting by cold, even down to 18 Fah., provided it does not congeal till it arrives at that temperature, which it will not do if it is kept perfectly still. Now the water, as it cools and condenses in the winter, sinks and carries its temperature with it, and as by hypothesis, the outlet to the bay is too shallow to allow it, by hydrostatic pressure, to flow towards the equator, it lies still at the bottom, and like the lake of Geneva, and some of the lakes of Scotland, whose waters at the bottom are about 41 or 42, it retains this temperature throughout the year. Quere—Does not Capt. Parry's observations prove, that the temperature of the freezing point of sea water is lowered by pressure?

cause the contraction is less at low temperatures than at high, let it be only one-third this quantity, that is $\frac{1}{276}$, then will the polar sea stand below the level of the equatorial $\frac{1}{276}$ of the whole depth.

If now we suppose, as Dr. Young has demonstrated from the velocity of the tidal wave, that the mean depth of the sea is 15840 feet, or three miles, we shall have the difference between the polar and equatorial level of the ocean, in case of equilibrium, 57 feet. But as a current above to the north, and one below to the south, must take place, the difference of level will be only half this quantity, or $27\frac{1}{2}$ feet, as stated above.

It may be objected to this theory, that no such superficial current, with the exception of the Gulf Stream, from the south to the north, is known; on the contrary, it is known that large masses of ice are found in latitudes far south of the arctic circle, which had floated south on the surface of the sea; that it is reasonable to suppose that the surface of the sea at the equator should stand constantly lower than that of polar seas, on account of the superior evaporation there, which is about six feet a year more in the former than in the latter climate, and that the current is actually known to be from the Hebrides to Spain. I grant the correctness of all these facts, and it may be added, that the immense quantity of ice and snow which is melted in the polar regions, during the summer, and which runs down into the ocean, must raise it a little above its mean level, and, as is proved by fact, actually invert the course of the current, in the eastern part of the Atlantic.

Notwithstanding all this, we have no reason to believe there is as much water flowing south on the eastern side of the Atlantic, as there is north on the western side at the surface; and besides all the latter facts which tend to diminish the superficial current to the north, operate to increase the current below towards the south.

Thus have I shown clearly, from theory, that the mean temperature of the Atlantic, in high latitudes, must be greater than that of the land on the same latitudes; consequently, the country to the leeward must have its mean annual temperature increased.

I come now to discuss the subject of rain, the most important of all meteoric phenomena, but one which has never yet been satisfactorily explained. Before I bring forward my own theory, I will observe that the only theory which has ever been advanced, possessing any plausibility, is that of Dr. Hutton.

The Doctor had observed that it frequently rained after he observed two currents of air moving for some time in different directions; and he deserves great credit for anticipating a fact which has since been amply demonstrated by experiment, that if two masses of air, of different temperatures, saturated with moisture, were mingled together, the united mass, with its common temperature, could not contain all the moisture which they contained before the union in the form of vapour, and of course there would be a precipitation. Hence the Doctor inferred, that it was only necessary for large masses of air, containing as much aqueous vapour, or nearly as much, as they could hold, to be mingled together, and rain would be the conse-

quence. This mingling he supposed actually took place when a current above and a current below blew for some time in different directions. Such was the plausibility of this theory that as soon as it was announced it was universally received by philosophers, and after the assumption of the mingling of airs of different temperatures producing deposition was demonstrated, the hypothesis was deemed to rest on the solid basis of fact. I admired the theory for its beauty, and never doubted for a moment the justness of it, until I discovered that the dew point frequently falls just before a rain.

This phenomenon was so contrary to my anticipations, that it could not fail to arrest my attention. If the air was simply mingled together between the two currents, blowing in different directions, I could see no reason for the great and rapid diminution of vapour in the regions below the clouds; for I have known the dew point to fall fourteen degrees, in the space of an hour, just before the rain commenced; though, as I afterwards ascertained, it was raining very hard during this very hour, within a few miles; and at another time it fell 20 degrees in 24 hours, before it began to rain, and when it began to rain it was about 30 degrees below the temperature of the air. By submitting these facts to a careful examination, I was enabled to form a true theory of rain, which I will communicate in a few words, after I shall have submitted Dr. Hutton's to a critical investigation. If any one will consider carefully, he may perceive that two currents of air may move in opposite directions, one above from the north, and one below from the south, for many hours and even days, without interfering much with each other's motion, or mingling together in any great degree. For the very central stratum of air between them will be stationary, and that on each side contiguous to this thin motionless stratum will move very slowly, and that a little further removed from the centre will move a little faster, until at the distance of a few yards, one hundred at most, the currents will move unimpeded and unmingled. I think any one who considers this case carefully and impartially, will grant that a stratum of 100 yards in thickness, is as much as could ever be mingled by different currents, moving even in opposite directions for many days. If this should be granted it will be manifest, that under the most favourable circumstances, when the currents of air mingled together were nearly saturated previous to their union—only a very thin cloud could be formed, which sinking down into the air below would be immediately dissolved if the temperature of this air should be at all above the dew point, which is always the case. But lest the position taken above should not be granted me, and some should think that a much larger quantity of air is mingled by different currents, let us put an extreme case, and suppose 1000 feet thick between the two currents, mingled together. Let us suppose also, what will certainly be allowed to be an extreme case, that these two currents differ in temperature 20 degrees, the temperature of the lower one being sixty, and the temperature of the upper one forty. Let us calculate, on known principles, how much water would be precipitated if both these currents had been saturated, previous to

mixture. By examining the table at the end of this article, it will be seen that a cubic inch of air whose dew point is sixty, contains .00338832 grains of water, and a cubic inch whose dew point is fifty, contains .00246714 grains; wherefore a cubic inch. whose temperature was reduced from 60 to 50 will precipitate the difference between these two quantities; that is, .00092118 grains, and the 500 feet in height will precipitate for every square inch of horizontal surface 6000 times .00092118, or 5.52708 grains. Moreover, a cubic inch of air of which the dew point is forty, contains .00178229 grains of water, and this subtracted from the quantity contained in a cubic inch whose dew point is fifty, as shown above, leaves .00068485 grains, which is the quantity that will be taken up for every square inch of horizontal surface and inch of perpendicular height of the 500 feet, whose temperature is elevated from 40° to 50°, giving a total of 4.10910 grains. Subtract this from 5.52708, the quantity precipitated from the 500 feet whose temperature was depressed, and there will remain 1.41798 grains, for the whole quantity of rain per square inch produced by this mixture of 1000 feet perpendicular height of air; not quite a grain and one-half on a square inch of surface: and even this small quantity would be diminished by a principle not taken into the account in the above calculation, namely, if two equal masses of air saturated with vapour of different temperatures be mingled together, the temperature of the mixture will be above the mean, for the vapour which is precipitated will raise the temperature by the evolution of latent caloric. Now as the lower portion of the atmosphere is never saturated with vapour, excepting when there is a fog, this minute portion of rain, as formed above, would not fall many feet in the air below, until the whole of it would be turned to vapour again, and none of it would reach the earth. Thus have I shown, by calculation from established principles, that rain could not be produced by the mixing of even much larger portions of atmospheric air, than are likely to be mixed by currents moving in different directions, even when these currents are saturated, a circumstance which seldom, or perhaps never, takes place in nature.

Another theory of less celebrity has been advanced by Mr. Morin. According to this author, rain can only be produced by compression of the air into a smaller space, as where it is forced against the sides of mountains by winds: or by refrigeration, caused by radiation from the cloud itself. Now most unfortunately for the first part of this theory—even if we grant that air is condensed by winds against the sides of mountains—experiment proves that the increase of heat by condensing air always *increases* its power of containing vapour.

And as to the second part of the theory—refrigeration by radiation from the cloud itself—it is defective in not accounting for the formation of the cloud. And even after a cloud is formed it is not at all likely that radiation from it is more rapid than from the surface of the earth in a clear night. Now we know from the phenomenon of dew that this radiation is too slow to account for the rapid formation of rain, as it occurs in nature.

It remains now to show the true cause of rain.

It is caused by the rush of the vapour itself, from the warm cur-

rent of air where it is denser and more tense, into the cold current of air, where it is immediately condensed into water.

The proof of this depends upon two facts. First, the great discovery of Dalton, that vapour forms an atmosphere of its own, the particles of which press only on each other; and consequently the superior strata of vapour are not buoyed up by the atmospheric air, but are supported entirely by the elasticity of the vapour below; and secondly, the actual depression of the dew point sometimes just before a rain.

What resistance air makes to vapour passing through it, or what is the greatest velocity, with which vapour can pass through a given mass of air, with a given tension, I have not been able to determine; but that it does pass with considerable velocity, the falling of the dew point 14° in one hour, affords abundant proof. Now this rush of vapour, into the cold current of air, answers the purpose of mingling the two currents together, in producing a common temperature; for the vapour in condensing into water in the cold current, gives out its latent heat, and when it falls down into the warmer current below, it cools it both by its own temperature being lower, and by evaporating again, provided the current below is not saturated; and this process goes on, raining above, and evaporating below; the point of deposition ascending, and the point of evaporation descending, until finally the rain reaches the earth, just as the stratum below is cooled down very near the dew point. And this is what takes place in every rain which is caused by a cold current above, and a warm current below, moving in different directions. Lest I might be misunderstood, by a cold current above, I mean one colder than that which is due to the elevation, that is, more than one degree colder for every hundred yards of elevation. If a rain should occur from the warm current being above, and the greater density of vapour in it, then the dew point will rise before the rain —the vapour below being rendered more dense by the pressure of the superior quantity of vapour above.

But there is another way by which rain is produced, and that is by the gradual increase of vapour in the atmosphere, until it rises into a temperature too low to correspond with its density; for the elasticity of vapour being much greater than that of air, its density does not decrease in ascending as fast as the temperature; which has been fully illustrated by Mr. Daniell. Rain is produced by this cause, generally, in those parts of the torrid zone where it rains every afternoon. The vapour goes on increasing both in quantity and elasticity from heat, till the hottest part of the day, soon after which, the upper part of the vaporic atmosphere begins to condense into water, by being thrust up by its own elasticity, into a region of cold, below its dew point; and as the air below is almost saturated, the drops of rain coming down into it, colder than the vapour there, condense a portion of this vapour, and thus increase in size until they descend into air, whose dew point is four or five degrees below the temperature of the air, which is generally the case near the surface of the earth. The rains near the equator, where there can hardly be cold

currents of air above, I think must all be caused in this way; but in our climate, we may expect to see rains often produced by cold currents of air overlapping warm ones—and such rains may always be predicted by the falling of the dew point. Every rain which occurred last April, was preceded by a fall of the dew point, from 6 to 24 hours before the commencement of the rain. During May, June and July, the rains have been of the equatorial character; the dew point has generally risen gradually to about seventy-two or three—that is within about two or three degrees of the mean temperature of the day, soon after which it commenced raining—sometimes with a slight fall of the dew point just before the rain—and only once with a sudden rise of three degrees, just before the rain commenced.

One phenomenon worth mentioning is, that on the 23d of June, I observed the clouds moving in different directions, the cirrus or upper clouds were coming from the south-west and the lower from the north. I have observed the upper clouds ever since that period (two days excepted, in which there were none,) constantly coming from that direction or from the west or some point between these extremes, and it is now the third of August. During this time the wind below has blown from every point of the compass. On the second of August, the lower clouds moved rapidly all day from the south, and the upper very slowly from the west, in the night there was some rain, and the next morning the lower clouds were moving rapidly from the north-west by north, and the upper clouds were moving from the south-west. Both these changes indicated to me that a great rain had taken place to the north. For when a large portion of vapour is condensed into rain, the diminution of volume which takes place can more easily be supplied by the subsidence of the air over the cloud, than it can by the air from the sides, which meets with much resistance from the obstacles on the surface of the earth. Therefore, the column of air immediately over the cloud will sink whilst it is raining, and the air at the surface of the atmosphere will run towards the cloud in all directions to restore equilibrium. Nor will it stop exactly at the moment when equilibrium is restored; it will accumulate where it was depressed before—the barometer will rise, and the air near the surface of the earth will be pressed outwards in all directions, from the region where it rained, and it will carry with it a great depression of the dew point—and some depression also of the temperature of the air. All these occurrences have taken place, and I anticipate, with great certainty, the report of a great rain on the night of the second of August, having occurred some place to the north of Philadelphia. I never yet have failed in predicting in what quarter the rain took place, when the phenomena were so marked as they have been in the present case. What I have said of the air being cooler, which blows from the rain, is not to be understood as recognising the principle, asserted by some writers, that it is colder because it comes from the upper regions of the air—*that*, I consider, I have demonstrated to be incorrect—it is colder because the rain descending from a great height

cools the stratum of air under the cloud, and this is chiefly the air that diffuses itself laterally. Indeed, as was shown before, the air in the cloud itself during the condensation of the vapour into drops of rain, is made warmer by the evolution of the latent caloric of the vapour, and this is one reason, perhaps the only one, that a pound of air (I do not say a gallon,) contains more caloric in the upper regions, than a pound below. If I have not been successful in convincing gentlemen who have embraced the opposite doctrine, I will ask them to explain why, on their principles, the air ceases to blow downwards immediately after it ceases to rain? If the whole column of air is colder and denser in consequence of having descended, than the air surrounding it, which has not descended, it should continue to blow downwards for ever, on the principle of greater specific gravity; for the air would manifestly flow in above, to preserve the level at the surface of the atmosphere.

I do not purpose to enter upon the subject of the trade winds at present; but I may simply observe that if I have been successful in showing that any theory, to account for the semi-diurnal fluctuations of the barometer, which assumes for its basis an alternate increase and diminution of gravitating matter over the barometer, is utterly without foundation, Professor Mitchell will discover that it contains also a refutation of his doctrine of vortexes. Besides if I understand the Professor's reasoning, and also his statement that the air at the surface of the atmosphere in the torrid zone runs eastwardly from noon and not westwardly, it will follow, that the barometer will stand higher some time after noon—than some time before it—which is not the fact.

The able reviewer (Professor Renwick, I believe,) of Mr. Daniell, Am. Quarterly Review, for March 1828, will discover also that he was too hasty in admitting, that as soon as the sun heats the air hotter than that in surrounding parts, the barometer falls, and the air begins to move in all directions towards the heated part, when in fact the first effect of heating air is the very reverse of this, both as to the barometer and the motion of the air; for the barometer will rise, and the motion of the air will be in all directions from the centre of greatest heat; and before the current can begin to move the other way, the air must have had time to run off above, so that the mass of gravitating matter may be diminished over the heated part. This latter effect however will not have time to take place, if the point of greatest heat moves with a velocity of a thousand miles an hour; for then, as was shown above, the air will not have time to spread itself over surrounding regions. Professor Olmsted of Yale College, who has written on the causes of hail, will also perceive how easy it is to account for this meteor by admitting the fact that dense vapour in one stratum of air may permeate another stratum where it is less dense, and very cold, with great rapidity—which, independent of theory, is rendered very probable by my observations on the dew point. The French also may learn to lay aside their paragreles; for unless they can prevent cold strata of air from overlapping warm ones, or prevent the vapour from shooting up into these

cold strata by its own elasticity, they will never be able to prevent hail.

As to the settling of moisture on a tumbler of cold well water, being a sign of rain, which is believed in many other places besides New Haven—there have been but seven days in the months of June and July, in which this would not have been given to any one who "seeketh after a sign," for the dew point was above fifty-five, the common temperature of well water, on every other day.

Notwithstanding the fallacy of the New Haven sign, it is to observations on the rise and fall of the dew point that we must look for means to predict the weather, and if this *point* is faithfully attended to, the result will be *success.*

The following table is extracted from the Edinburgh Encyclopedia—article Hygrometer, and is referred to in the preceding essay.

Temperature.	Weight, in grains, of the water in a cubic inch of vapour at the corresponding temperatures, Fah.	Temperature.	Weight, in grains, of the water in a cubic inch of vapour at the corresponding temperatures, Fah.	Temperature.	Weight, in grains, of the water in a cubic inch of vapour at the corresponding temperatures, Fah.
10	.00064161	34	.00146102	58	.00318197
11	.00066451	35	.00151051	59	.00328366
12	.00068825	36	.00156156	60	.00338832
13	.00071280	37	.00161424	61	.00349599
14	.00073810	38	.00166852	62	.00360679
15	.00076429	39	.00172454	63	.00372089
16	.00079136	40	.00178229	64	.00383826
17	.00081931	41	.00184189	65	.00395897
18	.00084819	42	.00190325	66	.00408317
19	.00087801	43	.00196651	67	.00421091
20	.00090882	44	.00203178	68	.00434230
21	.00094051	45	.00209899	69	.00447745
22	.00097337	46	.00216827	70	.00461639
23	.00100732	47	.00223878	71	.00475930
24	.00104235	48	.00231326	72	.00490628
25	.00107851	49	.00238903	73	.00505729
26	.00111588	50	.00246714	74	.00521259
27	.00115446	51	.00254757	75	.00537226
28	.00119420	52	.00263044	76	.00553634
29	.00123522	53	.00271574	77	.00570487
30	.00127758	54	.00280358	78	.00587810
31	.00132134	55	.00289415	79	.00605617
32	.00136636	56	.00298729	80	.00623919
33	.00141303	57	.00308325		

Description of Lieut. Cook's method of converting a Boat used for ordinary purposes into a Life Boat, at pleasure. Communicated by the inventor.

NOTWITHSTANDING that many plans for life boats for ships have been offered to the notice of the public, and that some have been partially introduced into the royal navy, to the best of the inventor's knowledge, there is not, at the present moment, a boat adopted by any service, whether British or foreign, naval or mercantile, calculated to despatch in a gale of wind, either to board a vessel at sea, to pick up a man who may have fallen overboard, or to send through a heavy surf to the shore, however great may be the case of emergency, although thousands of men have perished in such attempts!

To carry a life boat, in addition to the usual complement of boats, is out of the question; want of room alone is a sufficient objection. To take one, instead of another used for ordinary purposes, but which is not equally convenient and serviceable for common use, would also be objected to; while those plans which purposed to convert common boats into life boats, by moveable air tight cases, have been altogether thrown aside as cumbersome, and liable to the further objection of becoming waterlogged by the first sea that broke into them. It is then in the hope of having completely steered clear of the imperfections, which the opinion of nautical men attached to former ingenious contrivances, that the inventor respectfully offers to the notice of the maritime world at large, a very simple, cheap, and expeditious mode of converting *any* boat used for ordinary purposes, into a life boat, at pleasure, which cannot sink, nor become waterlogged, and into which, when a sea breaks, it will run out of itself, without exertion on the part of the crew. The invention is not one of problematical kind. It has been tried, officially tried, and most highly approved of, by an experienced and impartial officer, in a heavy surf on Deal beach, than which, a better place could not have been selected by the Hon. Navy Board. It was tried in the presence of officers of almost every rank in the navy, and in the presence of the first boatmen in the world, for whose use it is that Captain Pigot has recommended one to be kept on board the Gull-stream light. The Deal men declared it to be their conviction, that had they had a boat on this principle, 18 feet long, to have carried off in one of their large boats, to a vessel which was wrecked some time ago on the Goodwin Sands, the crew might have been saved; whereas, although eleven of the large boats succeeded in getting off, and actually anchored to windward of, and very near the wreck, it was only to have the melancholy and heart rending spectacle of beholding the crew drop from the rigging one after another into the sea; for owing to the broken water they dared not make any further effort to approach the perishing crew.

The invention consists principally in its having a neat shelf-piece of wood round the inside, level with the thwarts, about 5 inches wide by 1½ inch thick, containing a groove 2 inches wide, by half an inch

deep, in the middle of which are metal pins or bolts, 18 inches apart, fitted with screws and finger nuts at their upper ends. By these, light metal battens, (fluted underneath, and of the dimensions of the groove,) are firmly secured down on the edge of the canvass deck, when spread, as will be hereafter explained. On a level with the shelf-piece, four or more small brass circular screw scupper plates, 3 inches in diameter, and of the thickness of the plank, are neatly let into the sides of the boat, so as to be perfectly water tight. Two others, rather larger in diameter, are fitted through the stern. These are the only material alterations, and so far from their having an unsightly appearance they are really ornamental.

The above description will serve to show how very little in appearance this boat differs from those now used for ordinary purposes; while in five minutes it can be converted into a life boat, simply by spreading over the canvass deck, which is intended to keep the water from getting below the thwarts when a sea breaks into the boat, and which, in doubtful weather, or on detached service, may be kept in a cloak bag under the fore sheets. The water which does not run over the gunwale, escapes through the scupper holes. The edge of the canvass is bound all round with thin leather. In the binding eyelet holes are worked, 18 inches apart, to correspond with the position of the pins or bolts in the groove, over which they are hooked previously to placing on the battens, which, when screwed down into the groove, effectually prevent the water from passing the edge of the deck.

Each rower has a bag sufficiently large to contain his legs, his feet being placed on the stretcher: opposite to him is a spare bag, which, until wanted, may be turned inside out, and rolled up. There are also spare bags in the stern sheets. In very large boats, such as launchers, an aperture in the deck might be provided, properly secured, of sufficient dimensions to admit the body of a man, so that in a gale of wind, on detached service, a part of the crew—the canvass not being air-tight—might rest under cover: or in fine weather, in tropical climates, while thus employed, the whole of the boat might be covered over, excepting the stern sheets, to protect the men from the rays of the sun by day, and from the dews by night.

Lest the boat should leak, or in the event of any water oozing through the canvass, a hand pump is provided to keep her free: provision is likewise made for stepping the masts when required.

The fenders, made of cork shavings, and in a canvass tube the whole length of the boat, are in their construction, as well as in their mode of application, extremely simple, and were greatly approved of at Deal, not only on account of the security they afford to the boat in going alongside a wreck, but on account of the additional stability they give; and on account of their capability to support the crew, should a hole by any accident be knocked through the bottom of the boat.

An iron keel is also provided, so fitted outside, that in the event of an emergency, such as knocking a hole through the bottom of the boat, it can be instantaneously detatched by pulling a trigger; other

modes, however, of ballasting the boat, may be preferred by the different commanders of ships. The inventor names this for the information of such as may feel desirous of having it.

Explanation of the other inventions adverted to in Capt. Pigot's Report.

TACKLE FALL BLOCK.

Fig. 1.

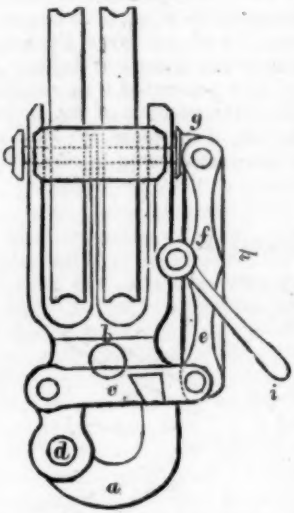
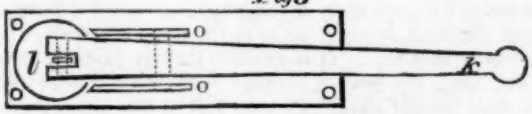


Fig. 1, is a view of the "tackle fall block," the object of which is to enable the crew to disengage the boat from the stern, or from both head and stern tackle falls in an instant, with such precision and certainty, as to remove the greatest part of the danger, connected with despatching a boat in a hurry, or in bad weather. *a* is the hook, the end of which rests firmly on the frame of the block *b;* *c* is the fork which keeps the hook from turning round at the joint *d;* *e*, *f*, are two legs, the former joined to the fork *c*, the latter to the pin of the block *g*, and are united together by a rule joint at *h*. While in their present straight position, they keep the fork, *c*, from liberating the hook *a;* but on the handle, *i*, being pulled, the legs *e*, *f*, form an angle like the dotted lines in fig. 4, when the fork releases the end of the hook, which instantaneously disengages the boat.

SHEET LIBERATOR.

Fig 2

*Fig*3



Figs. 2 and 3 are different views of the "sheet liberator." This simple apparatus effectually removes the necessity of employing men to stand by the sheets, when under a press of sail in boats in squally weather, and when their services may be much needed at the oars. It is no uncommon thing to hear of boats being found upset, with their sheets belayed. The inventor belonged to a frigate whose cutter was discovered in this situation; not one of the crew, consisting of an officer and thirteen men, escaped! all met with a watery grave. The liberator is fixed to the inside of the boat nearly in the position of fig. 3. The single part of the sheet is rove through the aperture *j*, and belayed forward. Should a sudden gust of wind take the sail, touch the lever *k*, and the shieve *l* turns round, relieving the boat, by releasing the bight of the sheet, without the danger of letting it fly. Although the shieve turns freely, it is prevented from coming off the spindle by a small nut. While in the position of fig. 2, the end of the spindle, *m*, rests in the socket, *n*, of the plate, the ears, *o*, of which, prevent the short end of the lever from being lifted up by the flapping of the sail.

BOAT'S GRIPES LIBERATOR.

Fig. 4.

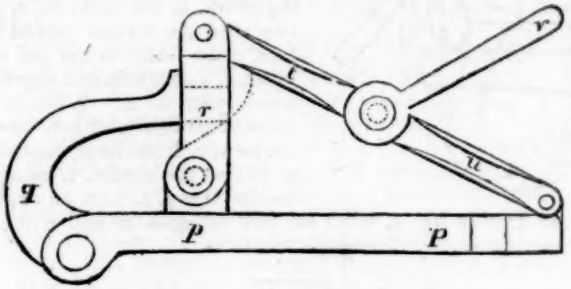


Fig. 4, is a representation of the "boat's gripes liberator." The plate *p*, is to be screwed or bolted to, and by, the edge of the mizen channels. The gripes being all of one piece, a thimble is to be seized into the middle or bight: this thimble is to be placed over the hook *q*, the end of which rests firmly on a stud cast in the plate *p*, to which the fork *r*, is bolted at *s*. The fork being kept in its perpendicular position by the two legs, *t* and *u*, as in fig. 1, it prevents the hook from turning round, until by pulling the handle *v*, it is permitted so to do. The two ends of the gripes are passed from the channels under the boat, brought up over the outer gunwale, and are set up by laniards as usual. It is evident that by casting off the bight or standing part, the boat is lowered down from under the gripes, and will clear herself should they be foul of the gunwale; whereas, by casting off the laniards, she is lowered down upon the gripes, and

is sure of being turned bottom upwards, should their being entangled pass unobserved.

To prevent corrosion no iron is used.

[*Rep. Pat. Inv.*

Remarks on the Novelty and Utility required in an Invention, to be the Subject of a Patent, in England.—To the Editor of the London Journal of Arts.

Sir,—It is a generally received doctrine that an invention, to be the subject of a Patent, must be new and useful, but it is not so well understood what constitutes novelty and utility, and how far either or both must exist.

Mr. Holroyd, in his law of Patents, p. 19, says, "whether an invention be new and useful is a question of fact proper for a jury."

"But it may be safely laid down that whatever be the nature of an invention, whether the merit of it rests on discovery, or on improvement, novelty and utility must exist in a material degree."

Now inventions do not always contain novelty and utility in equal portions; but in some, novelty is predominant, in others, utility; in some the novelty is very great, the utility trifling, in others the novelty is just discernible, and the useful effect very great. And though it is very certain that the sum of the two must be considerable, yet it is not easy to say where the sufficiency of each begins. That can only be ascertained by examination of the case upon record, where the sufficiency of novelty and utility of invention, has been brought into question.

In some cases where Patents have been supported, the novelty of the inventions has been trifling, and the merit of the Patents, on the strength of which they were supported, was their great utility. For instance, in Huddart's Patent for rope machinery, the novelty *per se* was only trifling, (see Davis's Patent cases, p. 265.)

But however trifling it might be, it had a very important effect, for by virtue of it, Captain Huddart made better cables and cordage, than had been made before, and the Patent was supported.

Again, in Daniell's Patent of 1824, the invention was plunging cloth, made in the common way, into hot water, after it had been dressed; this would seem a very small amount of novelty, or invention, but its effect was to produce a great improvement in the cloth; and assuming it not to have been practised before, the Patent was held good, and J. Fussell who used steam instead of hot water, for the same purpose, was held an infringer, and his Patent repealed. (See Appendix to the Report of the Committee of Patents, p. 211, the King against Fussell.)

Again, in Brunton's Patent, for improvements in ships' anchors and windlasses, and chain cables or moorings. The difference between Brunton's chain cables and Captain Brown's (who had preceded him) was a trifle. Captain Brown made his cables with twisted links, a wrought iron stay being fixed across the middle of the

opening of each link to keep them from collapsing. The links of Brunton's chain cables were not twisted, but made in the strongest form, and the stays across the links were made of cast iron, with broad ends adapted to the sides of the links, and embracing them. Brunton's links had come into general use for chain cables in place of Brown's, who himself had also adopted Brunton's links. The jury considered Brunton's chain cable new and useful; and afterwards on a motion for a new trial, the Lord Chief Justice, Abbot, confirmed their opinion as to the *sufficient novelty* of the chain cable, although he held the Patent bad on other grounds.—(See Appendix to Report of Patent Committee, p. 206.)

In all these cases the novelty was *per se* inconsiderable; but such as it was, by being combined skilfully with mechanism or processes known before, it went to produce important results in the shape of better cordage, chain cables, and cloth, than had been produced before; so that the public got really better things than they had had before the inventions: therefore, the inventions were held sufficiently new to comply with the law.

Absolute want of utility, or total failure in operation, of course avoids a Patent; as, however, such abortive inventions are rarely brought into court, there can be but few cases on record of decisions against them. But Patents have frequently been brought forward in court, grounded on inventions, in which the novelty has been more prominent than the utility, and in which the latter if not absolutely disproved, has failed to be established. Such patents have been generally brought forward merely as weapons of offence, to demolish other later patents for inventions founded on a similar principle, but differing somewhat in their practical combination and organization. As for instance, Balfour's Patent cited against Huddart's, in *Huddart* against *Grimshaw*.—(See Davies, p. 265, and Kirke and White's patent cited against Kneller's in *Hallett* against *Hague*.)

In most of these cases, the patents, which have taken a stand on their utility, have been maintained in spite of the preceding ones, which could not show utility; that is, the difference between the inventions, has generally been held sufficient to support the later patent.

It appears, therefore, from examination of the most important case, that utility is the quality most rigorously exacted by the law. For, first, an invention, however novel, which has not succeeded in practice, and has consequently been useless to the public, is not suffered to clog the operation of a subsequent invention which does succeed, and which though it may differ little from the first in appearance, differs from it in reality in something, by virtue of which it produces a better result, as in Huddart's case before cited, and in the late case of Hallett against Hague, tried in the Court of King's Bench, 26th of February, 1831.*

* In Hallett against Hague for an infringement of Kneller's patent,—Kirke and White had a patent, in 1822, "for a process for more rapid crystallization,

And, secondly, an invention which is held sufficiently new, and is also successful in practice, is supported against infringement by colourable evasions, as in the trials of Watt's patent; Clegg's patent for a gassmeter in Crossley against Beverley; and Daniell's patent in the King against Fussell.

Hence we may conclude, that when commanding utility is proved, a moderate share of novelty is sufficient to satisfy the law, because the utility is held *per se* novelty. For if an invention produces that which the public had not before, and which they prize highly, the *result* is the novelty, and it matters little in fact or in law, whether the means by which that valuable and new result is obtained, be trifling or not. It is not meant to be asserted, that a good machine or process possessing no novelty whatever, is merely on account of its goodness a subject for a patent; but it is barely possible, that a new effect, a result much better than previous results, can be obtained by virtue of no change whatever in the means; and if there is a change, although it be small, yet if its effects are great, and if it produces a beneficial result for the public, by giving them something real and useful, which they had not before, it may be expected, that a patent, grounded upon such change in the means, will stand firm as to novelty.

The truth is, that great novelty is becoming daily a more rare and difficult thing to attain;* and by reason of the minute subdivision that characterizes the present state of the useful arts, and the great extension of production in every branch of them, minute differences in the means employed, cause great difference in the results to the public. Hence there is value and merit in those minute differences; and it being daily more and more difficult to distinguish between them, we consider the results instead of the means, because

and for the evaporation of fluids at a comparatively low temperature." And Kneller took afterwards a patent for "certain improvements in evaporating sugar," &c.

It was contended by the defendants, who had the right of using Kirke and White's invention, that Kneller's specification described an invention essentially the same as Kirke and White's, although the construction of the one apparatus was somewhat different from that of the other; that hence, Kneller's apparatus came under Kirke and White's patent, and therefore, that they had a right to use Kneller's apparatus as a mere modification of their own.

It was proved that Kirke and White's invention had not come into use, and the defendants could not prove that it would answer the purpose, or was so effective as known methods of producing evaporation.

On the opposite side it was shown, that the difference between Kneller's invention and Kirke and White's, whether trifling or not, sufficed to make Kneller's apparatus answer the purpose; of which the proof was, that competent witnesses had tried it, and found it succeed upon experiment; and further, that it was in actual practice for trade at the plaintiff's sugar-house. Kneller's patent was supported against the infringement, and afterwards on a motion for a new trial, the verdict was confirmed by the court.

* This is not wonderful, considering that since the year 1675, 5539 patents have been granted (to say nothing of the inventions for which patents are not taken,) and considering further that hardly a month passes without two or three inventions on the same subject.

in the former, the good and the evil are magnified, so as to exhibit differences that the judgment can seize, as a microscope will make visible differences between things, that, to the naked eye, appear similar. [*London Jour. Arts.*

Cement from Iron Filings.

M. MAILTRE having reflected upon the action of vinegar in the preparation of the cement known as *mastic le limaille*, which is made of iron filings, garlic, and vinegar, so proportioned as to form a mass of moderate consistency, proposed to substitute for the vinegar sulphuric acid, diluted with water, in the proportion of one ounce to a little more than two pints of water, and to reject the garlic as useless. This alteration was soon adopted by all to whom he communicated it in Paris, and will save, in Paris alone, more than ten thousand francs annually. This cement is there employed to close the seams of stones with which terraces are covered. The iron filings becoming oxidized, occupy a larger space—their oxidation being facilitated by the action of the acid, and the joints become exactly closed.

[*Mechanics' Magazine.*

Mr. Collinge's Spherical Hinge.

SIR,—As it may not be generally known, that Mr. Collinge's spherical hinge is constructed upon the purest anatomical principles; and, as I am in the habit of illustrating my remarks upon the "mechanical structure of the human skeleton, compared with those of animals," in my popular lectures on that subject, by an exhibition of this invention, I deem it right to trouble you with a few observations on the structure of the hinge, and the joint it imitates. In doing so, I have no other object in view than to make more generally known the great ingenuity of the inventor; and to observe that this constitutes an illustration of the beneficial consequences that may result from the diffusion of that knowledge which teaches the component parts of animal bodies, which are well known to be constructed upon the truest mechanical principles.

The hip joint forms that species of articulation denominated, by anatomists, *enarthrosis*, or the ball and socket joint, by means of which the inferior extremity can be moved, in a circular direction, every way: the head of the thigh bone forming a large ball, which is received into a corresponding cup or cavity formed for its reception in the hip bone. Now this is exactly the mode of construction followed by Mr. Collinge, in his patent hinge. The resemblance is carried even farther. In the living joint a small quantity of synovia is secreted, by a series of diminutive glands, for the purpose of lubricating the surfaces in contact, and preventing the effects of friction. This is popularly, and not inaptly, called *joint oil;* though, in point of fact, it is not an animal oil, but resembles albumen, or white of egg. In Mr. Collinge's hinge there is, in the same way, an aper-

ture to admit a small portion of oil at the top, with another at the bottom, for its escape, when a new supply is required; and, to complete the imitation of nature, the whole is constructed perfectly air-tight.

I understand this species of hinge is now extensively used for gates, rudders, &c. for which purpose I should conceive it to be excellently adapted.

Yours, &c.

W. H. Dewhurst, *Surgeon.*

Professor of Human and Comparative Anatomy.

Fig. 1.

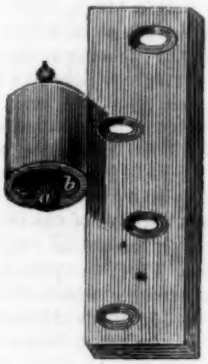


Fig. 2.

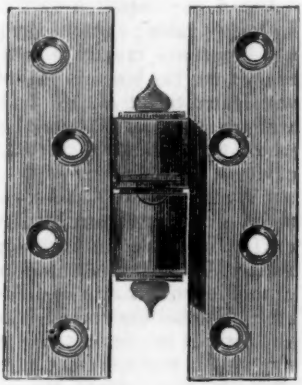


Fig. 3.

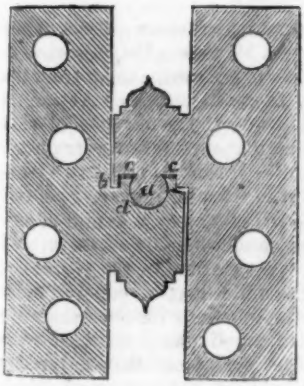
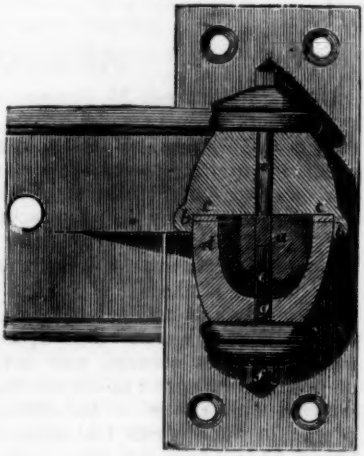


Fig. 4.



Description.

Fig. 1, is a perspective view of the upper half of a patent improved butt hinge; *a* shows the spherical bearing, which moves in a hemispherical cavity, made in the lower half of the hinge; *b* shows the recess, in which is placed a leather collar, to exclude air and dust.

Fig. 2, is an elevation of the same hinge, when the upper and lower halves are united.

Fig. 3, is a vertical section of Fig. 2; *a* the spherical bearing; *b*, *b*, the recess which contains the leather collar *c*, *c*; *d* the hemispherical cavity which contains the spherical bearing *a*, and also the oil necessary to lubricate the surfaces, and thereby prevent friction.

Fig. 4, represents a vertical section of parts of a hinge, suitable for heavy gates; *e* shows the groove formed in the spherical bearing, which permits the oil to flow up and lubricate the bearing surfaces every time the hinge is moved; *f* the head of a screw which may be withdrawn to let out the foul oil through the hole *g*; *h*, the head of a similar screw, that may be taken out when fresh oil is applied, which will run down through the hole *i*, and fill the hemispherical cavity *d*.

[We give above a more particular description of the invention so obligingly pointed out by Mr. Dewhurst, to the attention of our readers; it is, indeed, a most ingenious and very philosophical contrivance. We have been informed that, in the year 1798, the principle of this invention was applied to support an immense windmill shaft, on Sir Bethel Codrington's sugar estate in Antigua, where it remained, without inspection, nineteen years and a half; and that, when examined, it was apparent that the spherical pivot, and receiver, had never been in contact, the oil having kept their surfaces in a state of constant separation, and prevented all friction.—Ed. Mec. Mag.]

Protection of Firemen.

On the 4th of April M. Gregori communicated some details of the experiments recently made in Italy by the Marquess Origo, commandant of the firemen at Rome, with a view to guarantee them from the effects of entering houses while a prey to conflagration. Acting on the received opinion that the Romans employed a mixture of clay and vinegar to extinguish flames, he tried that mixture in every manner, but it produced no satisfactory result. He then dipped two complete suits of firemen's dresses, including boots, gloves, and two cowls, made of the same cloth as the dresses, in a solution of sulphate of alumine and sulphate of lime, and when dried, saturated them with soap water. Two firemen were clothed in these dresses, and their faces covered with incombustible masks, covered with cloth saturated with a saline solution; the openings for the eyes were covered with a web of amianthus, and small damp sponges were placed in their mouth and ears. Thus protected, they entered a house, 23 feet long and 3 feet wide, filled with burning wood, which they traversed ten times without the slightest injury. Their clothes

were not damaged, although they had remained fifteen minutes exposed to the action of the flames. The only effect produced on the men was the increase of the pulsation from 70 to 125. These dresses cost but two pounds sterling each; and are, therefore, in that respect more eligible than those composed of amianthus, as recommended by the Chevalier Aldini. M. Origo also extinguished flames of considerable violence by playing on them with the solution of sulphate of alumine and clay, by means of a common engine.—*Royal Academy of Sciences of Paris.*

Bibliographical Notices.

A Treatise on Mechanics, by Captain Henry Kater, Vice President, Royal Society, and Rev. Dionysius Lardner, LL.D. F. R. S. L. & E.

The above named work, which makes the fifth volume of Dr. Lardner's Cabinet Cyclopedia, has been reprinted both in Boston and Philadelphia. In this volume, the general properties of matter are explained in a clear and familiar manner; this, with The Laws of Motion; Gravitation; The Simple Machines, or Mechanic Powers; Wheel Work; The Means of Modifying Motion; Friction, and the Rigidity of Cords; The Strength of Materials; and an Investigation of the Nature and Uses of Balances and Pendulums, constitute the contents of the work. The different parts of the subject are treated without the introduction of mathematical formulæ, and the writers have been eminently successful in stepping down from the eminence on which they stand, to lead others up in the paths of science. The difficulty of doing this has been fully evinced in some of the numbers of the *Library of Useful Knowledge*, and other works intended for popular instruction, which, although they have evinced the learning of their authors, have too frequently been very illy adapted to the end proposed.

In the present work, the illustrations are very happily chosen; the beaten track has been departed from in many instances, so that those who have the popular treatises on mechanics upon their shelves, may still peruse this with real advantage. The chapter on Balances and Pendulums contains more direct and satisfactory information on these subjects than can be any where else found in the same compass.

The Academic Pioneer—conducted by the Editorial Committee of the Western Academic Institute, No. 1, Vol. I. Cincinnati, 1831.

We have just received the above pamphlet, and learn from it that the *Academic Institute*, consists of an association of teachers in the city of Cincinnati, formed for promoting the cause of general education. It is proposed by them to publish a monthly journal, at two dollars per annum, to diffuse such information as may be thought

most conducive to the object in view. The number before us is intended to form the first of the series. Its contents are an Address by C. B. M'Kee; The Constitution of the Society; A Discourse on Education, by the Rev. Dr. Bishop, President of Miami University; Address by Mr. Alexander Kinmont; Officers; Prospectus, &c.

Although we do not find in either of the published addresses any thing particularly novel, or striking, and have to regret that in point of composition they are not more creditable to their authors, yet we rejoice to see the teachers of youth uniting together in order to pursue measures calculated to enlarge their own sphere of knowledge, and, of course, to promote the improvement of the pupils committed to their care.

Knowledge for the People, or the Plain Why and Because. By John Timbs, Editor of 'Loconics' Arcana of Science and Arts, &c. No. 1, 18mo. 72 pages, 12½ cts. Lilly & Wart, Boston.

This is a very good title to a very indifferent book. The Whys are in most cases well enough, but the Becauses are, in numerous instances, *non sequeters*, or tend to "darken council with words without knowledge." This "Knowledge for the People" proceeds upon the supposition that the people already know all about the meaning of oxygen, hydrogen, and other chemical agents, as well as many other things, which, if they did know, such a work as the author *intended* to produce, would be of very little value.

The first question is, "*Why are coke and charcoal fires free from smoke?*" and the reply, "Because their moisture has been previously dissipated; this moisture producing the smoke of coal fires." Should some schoolboy ask Mr. Timbs, why it is then that moistened charcoal does not smoke when put upon the fire, he would have to imagine some other Because before he could answer the question. Is it the moisture in pitch, tar, resins and oils, which causes them to smoke when burnt?

The next Because introduces 'gases,' 'active caloric,' 'latent heat,' 'radient heat,' &c. &c. as familiar acquaintances of *the people.* We have a very intelligent community in the United States, but the great body of the operative classes of England must leave ours far in the rear, if they can derive instruction from Becauses thus constructed.

At page 4, we have "Why does sunshine extinguish a fire? Because the rays engage the oxygen which had hitherto supported the fire." We really know not what kind of engagement is here intended; for although the sun is introduced, and actively concerned in the contest, he does not throw any light upon the subject.

At page 5th we learn, that sulphur thrown upon a fire not only decomposes the atmospheric air, but absolutely *annihilates* it.

We might notice similar absurdities, on almost every page, intermixed, it is true, with some good Becauses, borrowed from Arnott, and others, and possibly, in some instances, supplied by the author himself.

Should the parts which are to follow manifest as little tact at explanation, and as much ignorance of the subjects to be explained, we hope that the American publishers will cause them to be expurgated and remodelled, before they are placed in the hands of our 'People.'

Since writing the foregoing we have seen two additional numbers, the 2nd and 3d, of the Plain Why and Because; the second is devoted to Zoology, the third to Origins and Antiquities. In point of general correctness they are to be preferred to the first number, but still we find in them but little to praise, or to approve. The same want of plainness in the Becauses, pervades the whole, and interferes with the utility of the work. The number on origins and antiquities, relates principally to manners and customs in England upon points but little understood, and possessing but little interest, in this country.

Scientific Tracts designed for instruction and entertainment, and adapted to Schools, Lyceums, and Families; conducted by Josiah Holbrook and others. $1 50 per annum for twenty-four numbers in 12mo. 24 pages each, Carter & Hendee, Boston.

We have seen thirteen numbers of this publication, and although upon the whole we think favourably of the work, there are some parts of it which are not skilfully managed, and several inaccuracies in point of fact. The second number, treating upon Geology, may be cited as a proof of the latter, as it ascribes to this science what belongs to mineralogy and chemistry. We are told, for example, that geology "has brought to view inexhaustible deposits of the material for the manufactory of the beautiful pigment under the name of chrome yellow;" and again, that "it has led to the establishment of a manufactory of epsom salts, where seven or eight hundred tons are made in a year!" It certainly was not by the aid of geology that either the chromate of iron, or the magnesian mineral referred to was discovered, nor did it suggest the preparations made from them.

In the number treating of the atmosphere we are told that "from the reflective power of the atmosphere we derive the delightful twilight before the sun rises, and after he has passed from our view." Here reflection is manifestly put for refraction. It is also said that "the atmosphere extends to nearly fifty miles from the surface of the earth." We are unacquainted with its extent, but have abundant reasons to conclude that it greatly exceeds the limit here assigned. Such a statement might be justified by a reference to the older writers on natural philosophy, but is altogether inconsistent with our present views upon this subject.

Notwithstanding these objections, there are some of the numbers of this work which we think merit unqualified praise. The 5th, on the Eye, and the 12th, on the Ear, both written by Jerome V. C. Smith, M. D., are particularly excellent.

Meteorological Observations for November, 1831.

Moon.	Days.	Therm. Sun rise.	Therm. 2 P.M.	Barometer. Sun rise.	Barometer. 2 P.M.	Dew point	Wind. Direction.	Wind. Force.	Water fallen in rain.	State of the weather, and Remarks.
				Inches.	Inches.				Inches.	
⊗	1	42°	52°	29.60	29.60	42	W.	Moderate.		Clear day.
	2	40	52	.80	.80	40	W.	do.		Clear—flying clouds.
	3	38	50	.90	.94	36	W.	do.		Clear day.
	4	34	54	.96	.96	36	N. S.	do.		White frost—clear—cl'dy
	5	38	52	30.00	30.00	39	do	do.		do do clear.
	6	32	50	.10	.10	36	W.	do.		do do clear; cl'dy.
	7	38	51	29.95	29.90	37	W.	do.		do do clear.
	8	43	52	30.10	30.10	31	W.	do.*		Clear day.
	9	43	56	.10	.10	41	W.	do.		Cloudy—hazy.
	10	48	64	.05	29.94	46	E.	Calm.		Fog—cloudy; rain night.
☽	11	58	60	30	.85	56	S.	do.		Cloudy—clear.
	12	36	48	.85	.83	35	SW. W.	High.		Clear—cloudy.
	13	36	48	30	.60	33	W.	Moderate.		Cloudy—clear.
	14	42	50	.55	.55	37	W. SW.	do.		Cloudy—flying clouds.
	15	40	48	.63	.65	32	W. NW.	Blustering.		Flying clouds—clear.
	16	34	46	.70	.70	27	NW.	do.		Frost—clear.
	17	32	48	.70	.70	31	NW.	Moderate.		Clear day.
	18	38	42	.60	.50	39	E.	do.	.31	Cloudy; drizzle. Rain n't.
	19	40	44	.60	.60	32	W.	Blustering.		Clear day.
	20	38	52	.60	.60	32	W.	Moderate.		White frost—clear.
	21	30	52	.30	.30	33	E. SE.	Blustering.	.45	Do clear; cl'y'd; rain even.
⊗	22	32	36	29.82	.00	25	NW.	do.		Snow, wind high. [ing.
	23	31	42	29.60	.75	26	W.	do.		Flying clouds.
	24	31	42	.90	.94	26	W.	do.		Clear day.
	25	29	42	30.00	30.00	26	NW. SE.	Moderate.		Cloudy; snow; hail.
	26	32	35	.00	29.85	34	SE. W.	do.	.03	Rain; cloudy.
	27	38	42	29.95	.90	40	SE. W.	Blustering.	.43	Flying cl'ds; clear; light
	28	33	33	.54	.60	32	NW.	do.		Clear day. [snow n't.
	29	25	27	.50	.50	10	W.	do.		Clear day.
	30	20	32	30.00	.90	12	W.	Moderate.		
	Mean	36.40	46.77	29.77	29.75	33.1			1.65	

	Thermometer.	Barometer.
Maximum height during the month,	64. on 10th.	30.10 on 6,8,&9th.
Minimum do.	20. on 30th.	28.82 on 22nd.
Mean do.	41.58	29.26

ERRATA.

Page 147, for continued from p. 40, read continued from p. 89.

There are some other obvious errors; at the distance of the editor from the press, this is unavoidable.

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ADDRESS

OF THE

COMMITTEE ON PREMIUMS

AND

EXHIBITIONS

OF THE

Franklin Institute of the State of Pennsylvania,

FOR THE

Promotion of the Mechanic Arts.

WITH

A LIST OF THE PREMIUMS

OFFERED TO COMPETITORS AT THE EXHIBITION TO BE HELD IN OCTOBER,

1831.

PHILADELPHIA:
J. Harding, Printer, 36 Carter's Alley.

1831

ADDRESS.

To the Manufacturers and Mechanics of the United States.

The Committee on Premiums and Exhibitions of the "Franklin Institute of the State of Pennsylvania, for the Promotion of the Mechanic Arts," inform the manufacturers, mechanics, and artizans of the United States, that the seventh exhibition of American manufactures will be held in the city of Philadelphia, on the 4th, 5th, 6th, and 7th of October, 1831, to which all persons are invited to send the products of their skill, ingenuity, and industry.

In order that the happy results which have attended former exhibitions may be secured for the one now announced, the Committee have determined to pursue the same system, with such modifications as increased experience leads them to believe will be beneficial.

The annexed regulations have been adopted for the occasion, and to these the Committee invite the particular attention of all who wish to deposit their goods.

It will be seen that the list of premiums is more varied and extensive than on former occasions. This measure has been adopted with a hope that it would enlist a larger share of public attention, and add to the value and interest of the exhibition. The regulations have been framed to secure the fullest impartiality on the part of the judges, and, it is hoped, will enable the Committee to accomplish the arduous and delicate task of discrimination, with that success which has attended the exertions of their predecessors. This is the most difficult, and perhaps the only unpleasant duty attached to the exhibitions. But the liberal confidence of the public, as well as the good sense and candour of the depositors, have hitherto contributed to relieve the task from many of its asperities; and the Committee hope, that as their exertions will be equally strenuous, their measures will be viewed with the same kindness and liberality.

The Institute have no other object in their exhibitions, than the promotion of American manufactures, by encouraging and stimulating our artizans, by creating a wholesome competition between those employed in similar branches, and by arraying together all that can contribute to remove the unfounded prejudices that are entertained against American manufactures.

The Committee take the liberty of reminding competitors for premiums, that the real merit of otherwise valuable specimens of art is much diminished by the injudicious introduction of heavy mouldings and carvings, incongruous forms and unharmonious colours. Experience proves that a discerning public have hitherto expressed a decided preference for those productions which combine simplicity with elegance.

Whilst it is desirable that such articles should be exhibited as are calculated to display the high attainments of our mechanics, and which manifest the utmost skill and delicacy, it is considered as equally, and, indeed, more important, that those which are of universal consumption, and which are manufactured in large quantities, should also appear, and that these should be of the kind and quality which are ordinarily made.

WILLIAM H. KEATING,
SAMUEL J. ROBBINS,
JAMES RONALDSON,
M. W. BALDWIN,
FREDERICK FRALEY,
JOSHUA G. HARKER,
ALEXANDER FERGUSON,
J. HENRY BULKLEY.

Com. on Prem. and Exhibitions.

Published by order of the Board of Managers.

M. D. Lewis, *Chairman.*

W. Hamilton, *Actuary.*

Philadelphia, February 10, 1831.

REGULATIONS.

1. All articles intended for competition must be deposited, at latest, on Monday, the third of October, at twelve o'clock, (noon.)
2. The judges shall be appointed on the twenty-sixth of September, and a list of them published in one or more newspapers.
3. To insure perfect impartiality, the Managers, and all firms or partnerships in which Managers are interested, shall be excluded from competition, and no Committee shall award a premium or compliment to any of its members.
4. No premium shall be awarded for an article that has received one at any other public exhibition; and none can receive a premium that is not equal in quality to the best articles of similar manufacture, presented at former exhibitions.
5. Whenever the price is made a condition of premium, the makers must engage to furnish the required quantity at the price affixed.
6. Proof of origin must be furnished, if required, for every specimen offered for premium.
7. The managers reserve to themselves the privilege of withholding a premium, whenever the article is deemed not to deserve it.
8. Should articles of extraordinary merit be presented, the managers will with pleasure extend premiums to them, although not embraced in the annexed list.
9. A full report of the premiums awarded, and of the principal goods exhibited, as well as a catalogue of all others, shall be published immediately after the exhibition. The reports of the judges, or parts of them, shall be appended to the general report, with their signatures.
10. All goods intended for competition, must be accompanied by an invoice stating the price.
11. Arrangements will be made to exhibit to advantage any working models that may be sent in for exhibition, and the managers respectfully invite contributions in this branch. Experience has shown the interest which the public take in them; and the managers are impressed with a conviction that the display of them is calculated to convey useful information. A careful and competent superintendent will be provided.

LIST OF PREMIUMS.

1831.

1. To the person who shall manufacture in the United States, the greatest quantity of iron from the ore, during the year ending September 1, 1831, using no other fuel than anthracite, the quantity not to be less than twenty tons. *A Gold Medal.*

2. To the person who shall manufacture in the United States, the greatest quantity of iron from the ore, during the year ending September 1, 1831, using no other fuel than the bituminous coal, the quantity not to be less than twenty tons. *A Gold Medal.*

3. To the person who shall invent the best grate or stove for the consumption of anthracite in cooking and other domestic purposes, in small families, one hundred dollars and *A Silver Medal.*

The grate or stove must be better and cheaper than any now in use. Its price must be under fifteen dollars, and the maker must obligate himself to furnish any required number, (not exceeding fifty,) at the same price and of equal quality. No grate or stove shall be received for competition, in which the fumes resulting from the combustion of the anthracite are brought into contact with the food to be cooked.

4. To the person who shall invent the best grate or stove for purposes similar to the preceding, but applicable for large families, hotels, &c. *A Silver Medal.*

The conditions to be the same as in the above, except that the price is not limited, but the cost for which it can be made will be taken into consideration in determining the merit of the claims presented.

5. To the inventor of the best constructed furnace and boiler, superior to any now in use, for generating steam, with anthracite, *A Silver Medal.*

Certificates will be required of the furnace having been in use some time, of the quantity of coal consumed, and of the effect produced.

6. To the maker of the best mountain barometer, which shall combine portability with great accuracy and durability. The scale to be divided by the aid of a vernier into at least the two thousandth part of an inch; with thermometers attached to the instrument, and divided according to Fahrenheit's scale, at least into degrees. The

case for enclosing the barometer in travelling, should serve as a stand for it when in use. *A Silver Medal.*

7. To the maker of the best theodolite. *A Silver Medal.*

The instrument to be provided with an achromatic telescope, furnished with a sun shade, reading both in the horizontal and vertical planes, by means of verniers to 30 seconds. The horizontal circle numbered to 360°. The graduated circles or arcs, and verniers, to be of silver, and all the movements by tangent screws; the instrument to be provided with fixed eye glasses.

The principles governing the construction, the results from actual trial, the appropriateness of the alloys, the propriety in the strength of the different parts, the entire weight and portableness, and the workmanship generally will decide the premium for this instrument.

8. To the maker of the best level. *A Silver Medal.*

The spirit glass to be about 8 inches in length, with an erect achromatic telescope provided with a sun shade.

The considerations governing the award in this case to be similar to those for the theodolite. The facilities in making the adjustment, and their permanency being of primary importance in this instrument, these considerations will have a corresponding influence in making the award.

9. To the maker of the best surveyor's compass. *A Silver Medal.*

The needle to be about six inches in length. The graduations to extend to half degrees, and without any apparatus to compensate for the variation. The compass box to be without letters, or even the *fleur de lis*, though the cardinal points should be represented by lines. The instrument to be provided with the tripod staff. The considerations determining the awards for the theodolite and level will decide the merits of this instrument.

10. To the maker of the best levelling staff. *A Silver Medal.*

The staff to be of wood, armed with shoes; graduated decimally, and to read by a vernier to the thousandth part of a foot; the least and greatest height the staff is capable of designating—the wood ensuring sufficient strength without inconvenient weight and liability to warp—and the facility and accuracy in use are the considerations which will govern the award in this case.

It is recommended that until the premiums on Nos. 7, 8, 9, and 10, are awarded, the makers' names be omitted from the instruments. Should these four premiums be awarded to the same maker, then, instead of giving him the four silver medals, he shall be entitled to *A Gold Medal.*

11. To the maker of the best rifle gun, iron mounted, with percussion lock and double triggers, to carry a ball of from 25 to 35 to the pound, and not to exceed $20 in price. *A Silver Medal.*

12. To the maker of the best rifle gun, with percussion lock and double triggers, to carry a ball of from 50 to 75 to the pound.
A Silver Medal.

No limitation being fixed to the price of the rifle in this premium, it is to be understood that in the award of it, the beauty as well as the excellence of the workmanship will be considered.

13. To the maker of the best double barreled fowling piece with percussion locks. *A Silver Medal.*

14. To the maker of the best oil from cotton seed, not less than one hundred gallons to be made. *A Silver Medal.*

15. To the maker of the best vegetable oil that will answer as a substitute for olive oil. It must be free from flavour. Not less than one hundred gallons to be made. *A Silver Medal.*

16. To the manufacturer of the the best margaric candles, not less than one thousand pounds to be made. *A Silver Medal.*

17. To the manufacturer of the best specimens of isinglass or fish glue, not less than twenty pounds in sheets to be exhibited.
A Silver Medal.

18. To the maker of the best ordinary glue, such as is used by cabinet makers, hatters, bookbinders, printers, &c. *A Silver Medal.*

The premium to be awarded for its fitness for all the above mentioned purposes.

19. To the maker of the best white vellum quarto post paper, made from the best No. 1 domestic rags, weighing not less than 8 lbs. per ream. Not less than one hundred reams to be made, and ten reams to be exhibited. *A Silver Medal.*

20. To the manufacturer of the best specimens of porcelain.
A Silver Medal.

The specimens to constitute a tea set of not less than seventy-two pieces. In awarding this premium, the texture of the ware, its colour, painting, gilding, and the elegance and tastefulness of the forms will be taken into consideration.

21. To the manufacturer of the best dinner set of blue and white China, of not less than one hundred and twenty pieces.
A Silver Medal.

The same considerations as in the preceding will influence the award of this premium.

22. To the maker of the best and most extensive variety of cut glass. *A Silver Medal.*

In awarding this premium, the judges will determine whether the glass is transparent and colourless, free from flaws or air bubbles—the forms tasteful and symmetric—the cutting regular and chaste—and the specimens combining most of these qualities in the greatest variety of pieces, shall be deemed the best.

23. To the maker of the best constructed lamp for illuminating the streets of a city. *A Silver Medal.*

24. To the maker of the best and most extensive variety of mantel, astral, or hanging lamps. *A Silver Medal.*

In the award of this premium, due regard will be paid to the construction of the lamp in all its parts—to propriety and elegance of form—and to the excellence of the workmanship in every respect.

25. To the maker of the best sideboard, to combine usefulness as well as beauty. *A Silver Medal.*

26. To the maker of the best ladies' cabinet secretary and book case, of wood, the growth or produce of the United States. *A Silver Medal.*

27. To the maker of the best set of cabinet chairs and sofa to match; not less than 12 chairs and one sofa to be exhibited, all of wood the growth and produce of the United States. *A Silver Medal.*

28. To the apprentice who exhibits the best specimen of his own work in mortising and tenoning wood, not less than 12 mortises and tenons to fit together, also six veneered columns, suitable for bureaus or sideboards. *A Silver Medal.*

No premium will be awarded for the cabinet ware unless the design be chaste and elegant, and the workmanship equal to the best exhibited on former occasions.

29. For the best cast iron hollowware, tinned inside, for culinary purposes, to be equal to the best British goods of the same description, not less than one dozen pieces to be exhibited. *A Silver Medal.*

30. For the best brass battery kettles, equal to those made in Great Britain, not less than one dozen to be exhibited. *A Silver Medal.*

31. For the best bell metal kettles, not less than one dozen to be exhibited. *A Silver Medal.*

32. For the best iron wire of assorted numbers, from 7 to 25, not less than 200 lbs. to be exhibited. *A Silver Medal.*

33. For the best brass wire of assorted numbers, from 7 to 25, not less than 200 lbs. to be exhibited. *A Silver Medal.*

34. For the best card wire of assorted numbers, from 30 to 35, not less than 200 lbs. to be exhibited. *A Silver Medal.*

35. For the best cast steel hand and pannel saws, 26 inches long, not less than one dozen of each to be exhibited. *A Silver Medal.*

36. For the best cast steel iron back saws assorted, from 10 to 18 inches long, not less than two dozen to be exhibited. *A Silver Medal.*

37. For the best tea set of silver plated goods. *A Silver Medal.*

38. For the best set of table cutlery, to consist of not less than 51 pieces. *A Silver Medal.*

39. For the best anvils, equal to the mouse hole anvils, assorted from 1 to 2 cwt. not less than five anvils to be exhibited.
A Silver Medal.

40. For the best japanned tea trays, the japanning to be done upon American sheet iron, not less than six trays to be exhibited.
A Silver Medal.

41. For the best bar iron, from 2 to 5 inches square, and round iron from 2 to 5 inches in diameter, not less than 5 tons of each to be made, and 5 cwt. of each to be exhibited. *A Silver Medal.*

42. For the best cast steel of assorted sizes, not less than 2 cwt. to be exhibited. *A Silver Medal.*

43. For the best sheet iron, equal to that made in Russia, not less than 24 sheets to be exhibited. *A Silver Medal.*

44. For the best cast steel files, from 6 to 14 inches in length, one dozen of each, rough, bastard and smooth cut, to be exhibited.
A Silver Medal.

45. For the best smiths' vices, equal to those known by the name of tower vices, not less than five to be exhibited, and to weigh from 30 to 80 lbs. *A Silver Medal.*

46. For the best trowels for the use of stone masons, bricklayers, and plasterers, not less than twelve of each to be exhibited.
A Silver Medal.

47. For the best sample of drab and olive bang-up cord and velveteen, not less than two hundred yards to be exhibited.
A Silver Medal.

48. For the best sample of green and red canton flannel, not less than twenty-six inches wide, and two hundred yards to be exhibited. *A Silver Medal.*

49. For the best sample of white canton flannel, twenty-six inches wide, not less than two hundred yards to be exhibited, and to be superior to any before offered. *A Silver Medal.*

50. For the best sample of $\frac{6}{4}$ cambric muslin, made of yarns from number forty to one hundred and twenty, not less than five pieces of twelve yards each to be exhibited. *A Silver Medal.*

51. For the best sample of $\frac{4}{4}$ book muslin, made of yarns from No. one hundred to two hundred, not less than five pieces, of ten yards each, to be exhibited. *A Silver Medal.*

52. For the best sample of plain and figured laces and lace veils, to be exclusively of American manufacture. *A Silver Medal.*

53. For the best sample of furniture chintz, not less than four colours, and not less than five pieces of twenty-eight yards each, to be exhibited. *A Silver Medal.*

54. For the best sample of rich chintz prints, for ladies' dresses, not less than three colours, and not less than five pieces of twenty-eight yards each, to be exhibited. *A Silver Medal.*

55. For the best sample of single coloured prints, the price not to exceed eleven and a half cents per yard, not less than five pieces, of twenty eight yards each, to be exhibited. *A Silver Medal.*

56. For the best sample of single or two coloured prints, the price not to exceed fifteen cents per yard, not less than five pieces of twenty-eight yards each, to be exhibited. *A Silver Medal.*

57. For the best sample of two blue prints, not less than five pieces of twenty-eight yards each, to be exhibited. *A Silver Medal.*

58. For the best sample of printed cotton handkerchiefs, not less than ten dozen to be exhibited, and to measure square.
A Silver Medal.

Colours of all printed goods to be permanent.

59. For the best sample of plain chintz or coloured cambrics, some of which to be pink, light blue, green, and yellow, not less than ten pieces to be exhibited. *A Silver Medal.*

60. For the best sample of $\frac{4}{4}$ fancy gingham in imitation of the Scotch, of yarn No. 45 or upwards, not less than ten pieces of stripes and checks of equal lengths, to be exhibited. *A Silver Medal.*

61. For the best sample of indigo blue checks, not less than one hundred and fifty yards to be exhibited. *A Silver Medal.*

62. For the best sample of pantaloon stuffs $\frac{3}{4}$ to $\frac{7}{8}$ wide, all cotton, or cotton and wool, not less than two hundred yards to be exhibited.
A Silver Medal.

63. For the best sample of ladies' white cotton hose, not less than five dozen to be exhibited, and to be put up as well as the imported article. *A Silver Medal.*

64. For the best sample of ladies' black worsted hose, not less than two dozen to be exhibited, and to be put up as well as the imported article. *A Silver Medal.*

Silks.

65. For the best sample of sewing silk, blue, black, and assorted colours, not less than five pounds of sixteen ounces each, to be exhibited, to be put up after the manner of the Italian.
A Silver Medal.

66. For the best sample of silk hosiery, not less than one dozen to be exhibited. *A Silver Medal.*

67. For the best sample of black and white silk gloves, not less than one dozen pair of each colour to be exhibited.
A Silver Medal.

68. For the best sample of printed silk pocket handkerchiefs, to measure square, not less than three pieces, of seven handkerchiefs each, to be exhibited. *A Silver Medal.*

N. B. The material and manufacture of all silk goods to be exclusively American.

69. For the best specimen of full blue satinett, indigo dye, to measure at least twenty-eight inches wide, and not less than five pieces to be exhibited. *A Silver Medal.*

70. For the best specimen of fancy mixed satinett, to measure at least twenty-eight inches wide, and not less than five pieces to be exhibited. *A Silver Medal.*

71. For the best sample of satinett, the price not to exceed sixty-five cents per yard, and not less than five pieces of different colours to be exhibited. *A Silver Medal.*

72. For the best sample of blue, black, and fancy, single or double milled cassimeres, all to be permanent colours, not less than five pieces to be exhibited. *A Silver Medal.*

73. For the best sample of superfine blue or black cloth, permanent colours, not less than sixty inches wide, and exclusively of American manufacture. *A Silver Medal.*

74. For the best sample of blue cloth, indigo dye, to measure not less than fifty-four inches wide, price not to exceed three dollars per yard, and not less than forty yards to be exhibited. *A Silver Medal.*

75. For the best sample of fancy coloured cloth, to measure not less than fifty-four inches wide, and not less than forty yards to be exhibited. *A Silver Medal.*

76. For the best sample of milled drab cloth, to measure not less than forty-five inches wide, price not to exceed one dollar and fifty cents per yard, and not less than fifty yards to be exhibited. *A Silver Medal.*

77. For the best samples of red and green baize, not less than one hundred yards to be exhibited. *A Silver Medal.*

78. For the best sample of fine white gauze flannel, not less than three pieces of forty-six yards each, or six half pieces of twenty-three yards each, to be exhibited. *A Silver Medal.*

79. For the best sample of worsted stuff goods, coloured or black, not less than one hundred and twenty yards to be exhibited, and if bombazetts, to be in pieces of twenty-eight yards each. *A Silver Medal.*

80. For the best sample of ingrain carpeting, thirty-six inches wide, not less than sixty yards to be exhibited. *A Silver Medal.*

81. For the best sample of Venetian carpeting, twenty-seven or thirty-six inches wide, not less than sixty yards to be exhibited. *A Silver Medal.*

82. For the best sample of Brussels carpeting, not less than forty yards to be exhibited. *A Silver Medal.*

83. For the best sample of straw bonnets, if superior to any before exhibited. *A Silver Medal.*

84. For the best sample of three point blankets, in imitation of the Mackinaw blankets, to measure not less than six feet six inches long, by five feet six inches wide, to weigh at least eight pounds six ounces per pair, two pair to be exhibited. *A Silver Medal.*

The stripe and point marks to be clear indigo blue.

85. For the best bed blankets, either rose or whitney, to measure at least $\frac{10}{4}$ wide, by $\frac{12}{4}$ long. *A Silver Medal.*

86. For the best cotton blankets, at least twelve pair to be exhibited. *A Silver Medal.*

87. To the manufacturer of the best specimen of linen shirting, to be equal to the Irish, not less than fifty yards to be exhibited. *A Silver Medal.*

88. For the best specimen of leather in imitation of Russia, one dozen skins to be exhibited. *A Silver Medal.*

89. To the manufacturer of the best plate glass, not less than fifty square feet to be exhibited. *A Silver Medal.*